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ABSTRACT

Papers from the meeting "Prevention Research: Deterring Drug Abuse Among Children and Adolescents" which focused on social skills and social inoculation approaches and also included a contrasting cognitive-developmental approach are presented in this document. These papers are included: (1) "Overview of Drug Abuse Prevention Research," (Catherine S. Bell and Robert J. Battjes); (2) "Personal and Social Skills Training: Cognitive-Behavioral Approaches to Substance Abuse Prevention," (Gilbert J. Botvin and Thomas A. Wills); (3) "Social and Personal Skills Training Programs for Smoking Prevention: Critique and Directions for Future Research," (Russello E. Glasgow and Kevin D. Mc Caul); (4) "What We know About the Social Influences Approach to Smoking Prevention: Review and Recommendations," (Brian R. Flay); (5) "The Social Influences Approach to Smoking Prevention and Progress Toward an Integrated Smoking Elimination Strategy: A Critical Commentary," (Harry A. Lando); (6) "A Cognitive Developmental Approach to Smoking Prevention," (Kathleen Glynn, Howard Leventhal, and Robert Hirschman); (7) "The Cognitive Developmental Model and Other Alternatives to the Social Skills Deficit Model of Smoking Onset," (William J. McCarthy); (8) "Methodological Issues in Research on Smoking Prevention," (Anthony Biglan and Dennis V. Ary); (9) "Priorities in Research in Smoking Prevention," (Thomas D. Cook); and (10) "Future Directions in Drug Abuse Prevention Research," (Robert J. Battjes and Catherine S. Bell). (ABL)

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Prevention Research: Deterring Drug Abuse Among Children and Adolescents

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U. S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Public Health Service • Alcohol, Drug Abuse, and Mental Health Administration

Prevention Research: Deterring Drug Abuse Among Children and Adolescents

Editors:

Catherine S. Bell, M.S.

Robert Battjes, D.S.W.

Division of Clinical Research
National Institute on Drug Abuse

NIDA Research Monograph 63 A RAUS Review Report

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**Prevention Research:
Deterring Drug Abuse Among
Children and Adolescents**

ACKNOWLEDGMENT

This monograph is based upon papers and discussion from the RAUS review on prevention research which took place on April 5 and 6, 1984, at Rockville, Maryland. The review meeting was sponsored by the Office of Science and the Division of Clinical Research, National Institute on Drug Abuse.

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Foreword

In 1983 a Research Analysis and Utilization System (RAUS) review entitled "Preventing Adolescent Drug Abuse: Intervention Strategies" (Research Monograph 47) considered a variety of prevention approaches. As an outgrowth of that meeting, it was determined that two closely-related approaches, social skills and social inoculation, warranted more intensive review. These school-based strategies, developed first in smoking prevention and later applied to other substances, had been studied extensively by a number of investigators funded by the National Institute on Drug Abuse (NIDA) and other components of the Public Health Service with encouraging preliminary results.

"Prevention Research: Deterring Drug Abuse Among Children and Adolescents" was, therefore, selected as a subject for a comprehensive RAUS review in 1984. It focused on social skills and social inoculation approaches and also included a contrasting cognitive-developmental approach. The meeting was designed to thoroughly review these promising smoking prevention strategies, consider their appropriateness for transference to other substances, and clarify future research directions.

Charles Gruder served as the scientific moderator of the meeting. Catherine Bell and Robert Battjes were the NIDA staff responsible for organizing the meeting. Jacqueline Ludford, Chief, Research Analysis Branch, Office of Science, is the RAUS coordinator for NIDA.

Charles R. Schuster, Ph.D.
Director
National Institute on Drug Abuse

Preface

On April 5-6, 1984, the National Institute on Drug Abuse (NIDA) hosted a review meeting under the aegis of the Research Analysis and Utilization System (RAUS) on "Prevention Research: Detering Drug Abuse Among Children and Adolescents." This meeting shared the goals of all RAUS reviews, namely the evaluation of federally funded research, dissemination of conclusions from the evaluation, provision of feedback to NIDA planners, and identification of high priority targets for future research.

This conference can perhaps best be understood in context--it followed a 1983 RAUS conference which also reviewed adolescent drug abuse prevention research (Preventing Adolescent Drug Abuse: Intervention Strategies, NIDA Research Monograph 47). In the final chapter of the monograph reporting on the 1983 review, Leukefeld and Moskowitz noted: "In sum, research on preventive interventions is in its infancy due to theoretical and methodological inadequacies" (p. 253). With respect to the dissemination and implementation of existing preventive interventions, they reported that, on the one hand, conferees desired to begin implementing promising prevention programs, but, on the other hand, they were reluctant to recommend any particular program for dissemination.

It seems appropriate to take these conclusions as the starting point for the present report on the 1984 RAUS review, which looked more closely at selected promising prevention approaches. How far have investigators come, i.e., are we closer to the ultimate goals of implementation and dissemination? Do investigators and observers still agree that the research base is inadequate and further research is required? Are research recommendations the same or have they changed?

The chapters in this monograph provide a thorough review of research data on the most promising preventive intervention approaches. This information will go a long way toward answering our questions. Moreover, this process has aided in the identification of new directions for future research.

Charles L. Gruder, Ph.D.
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Overview of Drug Abuse Prevention Research

Catherine S. Bell, M.S., and Robert J. Battjes, D.S.W.

Public concern over the issue of drug abuse among children and adolescents has stimulated a major effort on the part of researchers and clinicians to identify effective ways of deterring or delaying onset of this behavior. Traditional health education approaches had proven largely unsuccessful in reducing rates of drug abuse. Thus new approaches were sought to address this problem. Encouraging results have been reported from research studies based on psychosocial models of behavior. Originally applied to the prevention of tobacco use, these interventions are currently being adapted to other drug abuse behaviors (e.g., alcohol and marijuana use), and preliminary findings are most promising.

OVERVIEW

Although the purpose of this monograph is to identify and delineate the currently available research knowledge relevant to deterring adolescent drug abuse in general and cigarette smoking in particular, it is appropriate first to provide the context within which the chapters were developed.

In 1982 the National Institute on Drug Abuse (NIDA) established the Prevention Research Branch in the Division of Clinical Research. The mandate of this unit includes two areas of research: etiology and preventive interventions. The former seeks to expand understanding of antecedent factors which inhibit or contribute to the risk of drug abuse in later life. Etiological research findings are intended to assist in the development or refinement of preventive interventions. The second major area, preventive intervention research, examines the efficacy of a wide variety of strategies to deter or delay the onset of drug abuse behaviors. This research largely focuses on youth; however, other groups at above average risk for drug abuse are also included in the research mandate. Etiological research has long been an integral part of NIDA's drug abuse research program, whereas preventive intervention research has emerged as a priority area only within the past few years.

In establishing the Prevention Research Branch, NIDA demonstrated its commitment to expand the Institute's prevention program and recognized the importance of a close linkage between research and application. In order to stimulate investigator-initiated grant applications in priority areas, the Branch has issued a number of research grant announcements:

- o Drug Abuse Prevention Research Announcement
- o Family Therapy and Prevention Research Announcement
- o New Investigator Research Award in Prevention
- o Community Prevention Research in Alcohol and Drug Abuse
- o Prevention of Alcohol, Drug, and Mental Health (ADM) Disorders at the Worksite

To insure that interventions designed to prevent tobacco use had been thoroughly tested, that they were appropriate for transference to other drugs of abuse, and to identify potential problems, provide solutions, and direct future research, NIDA held a 2-day working conference on April 5 and 6, 1984.

This meeting was part of NIDA's Research Analysis and Utilization System (RAUS). RAUS reviews are designed to serve four functions:

- o Collect and systematically classify the findings of all intramural and extramural research supported by NIDA;
- o Evaluate the findings in selected areas of particular interest and formulate a state-of-the-art review by a panel of scientific peers;
- o Disseminate findings to researchers in the field and to administrators, planners instructors, and other interested persons;
- o Provide a feedback mechanism to NIDA staff and planners so that the administration and monitoring of the NIDA research program reflect the very latest knowledge gleaned from research in the field.

This monograph is a product of that conference. The participants included representatives of three major psychosocial intervention approaches: social skills, social inoculation, and cognitive-developmental. Discussants included social scientists with broad behavioral and clinical experience in the related fields of smoking cessation, stress management, and alcohol and drug abuse.

As a complement to this meeting, NIDA sponsored a conference entitled "Etiology of Drug Abuse: Implications for Prevention" on April 24 and 25, 1984. Research Monograph 56 reports the findings of that conference (Jones and Battjes 1985).

SCIENTIFIC BACKGROUND

During the past 20 years, Americans have become increasingly aware

of and concerned about the level of drug involvement among youth. At the end of the 1960s, the use of illicit drugs among teenagers and young adults was recognized as a major epidemic. Large segments of the nation's youth had begun to experiment with marijuana, psychedelics, and psychoactive drugs such as phencyclidine (PCP). Moreover, misuse and abuse of prescription drugs (e.g., tranquilizers and stimulants) was on the increase. By the mid-1970s, illicit drug experimentation seem to have become synonymous with other "rites of passage" into adulthood.

Although rates of drug use for most substances declined during the late 1970s and early 1980s, drug use among adolescents and young adults continues to be a significant problem. As measured by NIDA's High School Senior Survey, in 1984, 19% of the respondents reported having smoked cigarettes daily, 5% were drinking alcohol daily, and 39% had five or more drinks in a row at least once in the past 2 weeks. Approximately 25% of high school seniors reported current use of marijuana. Moreover, 62%, or roughly two-thirds of the respondents, had tried an illicit drug (Johnston et al. 1985). Table 1 provides an overview of trends in 30-day prevalence of 16 types of drugs and table 2 provides annual prevalence.

EARLY PREVENTIVE INTERVENTIONS

Historically, drug abuse prevention programs were founded on the theoretical assumption that children and adolescents used drugs because they were ignorant of the consequences of such use. Failure to recognize negative consequences resulted, according to this theory, in neutral or even favorable attitudes regarding experimentation and/or regular use.

During the 1960s, drug education programs focused on providing information. These programs frequently contained "fear arousal messages" regarding the health and social consequences of such use. Perhaps of more significance, youth reported that the messages themselves lacked credibility.

By the 1970s, social scientists had begun to address interpersonal and intrapersonal factors that influenced drug abuse behaviors among children and adolescents (Goodstadt 1975). Correlational studies found drug abuse was associated with attitudes, beliefs, and values, as well as other personality factors such as feelings of self-esteem, self-reliance, and alienation. One prevention approach which evolved from this research was affective education. Rather than focusing on drug abuse behaviors, affective education focused on the factors associated with use, attempting to eliminate the reasons for using drugs by creating a school climate which was supportive of students' social and emotional needs. These programs frequently focused on training the students in effective decision-making skills. Specific techniques included: (1) values clarification; (2) analysis of consequences of behavioral choices; and (3) identification of alternative behaviors consistent with one's values and beliefs regarding drug abuse.

TABLE 1
Trends in Annual Prevalence of Sixteen Types of Drugs

	Percent who used in last twelve months										%3-yr change
	Class of 1973	Class of 1976	Class of 1977	Class of 1978	Class of 1979	Class of 1980	Class of 1981	Class of 1982	Class of 1983	Class of 1984	
Approx. N =	(1900)	(1500)	(1710)	(1780)	(1350)	(1360)	(1750)	(1770)	(1630)	(1560)	
Marijuana/Hashish	40.0	38.5	37.6	39.2	30.8	38.8	36.1	39.3	32.3	30.0	-2.3x
Inhalants ^a	NA	3.0	3.7	3.1	3.4	3.6	3.1	3.3	3.3	3.1	+0.8x
Inhalants Adjusted ^b	NA	NA	NA	NA	9.2	7.8	6.0	6.6	6.7	7.0	+1.2x
Amyl & Butyl Nitrites ^c	NA	NA	NA	NA	6.5	5.7	3.7	3.6	3.6	3.0	+0.8x
Hallucinogens	11.2	9.8	8.8	9.6	9.9	9.3	9.0	8.1	7.3	6.5	-0.8x
Hallucinogens Adjusted ^d	NA	NA	NA	NA	12.8	10.6	10.1	9.3	9.3	7.9	-1.4x
LSD ^e	7.2	6.3	5.5	6.3	6.6	6.3	6.5	6.1	5.4	4.7	-0.7x
PCP ^f	NA	NA	NA	NA	7.0	4.9	3.2	2.2	2.6	2.3	-0.3x
Cocaine	5.6	6.0	7.2	9.0	12.0	12.3	12.4	11.3	11.8	11.6	+0.2x
Heroin	1.0	0.8	0.8	0.8	0.5	0.5	0.5	0.6	0.6	0.5	-0.1x
Other opiates ^g	5.7	5.7	6.4	6.0	6.2	6.3	5.9	5.3	5.1	5.2	+0.1x
Stimulants ^h	16.2	15.8	16.3	17.1	18.3	20.8	26.0	26.1	24.6	NA	NA
Stimulants Adjusted ^{b,i}	NA	NA	NA	NA	NA	NA	NA	20.3	17.9	17.7	-0.2x
Sedatives ^j	11.7	10.7	10.8	9.9	9.9	10.3	10.5	9.1	7.9	6.6	-1.3xx
Barbiturates ^k	10.7	9.6	9.3	8.1	7.3	6.8	6.6	5.5	5.2	4.9	-0.3x
Methaqualone ^l	5.1	3.7	5.2	4.9	5.9	7.2	7.6	6.8	5.4	3.8	-1.6xxxx
Tranquilizers ^m	10.6	10.3	10.8	9.9	9.6	8.7	8.0	7.0	6.9	6.1	-0.8x
Alcohol	39.8	37.7	37.0	37.7	38.1	37.9	37.0	36.5	37.3	36.0	-1.3x
Cigarettes	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA

NOTE: Level of significance of difference between the two most recent classes:
x = .05, xx = .01, xxx = .001.

NA indicates data not available.

^aData based on four questionnaire forms. N is four-fifths of N indicated.

^bAdjusted for underreporting of amyl and butyl nitrites (see text).

^cData based on a single questionnaire form. N is one-fifth of N indicated.

^dAdjusted for underreporting of PCP (see text).

^eOnly drug use which was not under a doctor's orders is included here.

^fAdjusted for overreporting of the non-prescription stimulants.

TABLE 2
Trends in Lifetime Prevalence of Sixteen Types of Drugs

	Percent ever used										'83-'84 change
	Class of 1975	Class of 1976	Class of 1977	Class of 1978	Class of 1979	Class of 1980	Class of 1981	Class of 1982	Class of 1983	Class of 1984	
Approx. N =	(9400)	(13400)	(17100)	(17800)	(13500)	(15900)	(17300)	(17700)	(16300)	(15900)	
Marijuana/Hashish	47.3	52.8	56.4	59.2	60.4	60.3	59.3	58.7	57.0	54.9	-2.1
Inhalants ^d	NA	10.3	11.1	12.0	12.7	11.9	12.3	12.8	13.6	14.4	+0.8
Inhalants Adjusted ^d	NA	NA	NA	NA	18.7	17.0	17.4	18.0	18.0	19.0	+0.2
Amyl & Butyl Nitrites ^e	NA	NA	NA	NA	11.1	11.1	10.1	9.8	8.4	8.1	-0.3
Hallucinogens	16.3	15.1	13.9	14.3	14.1	13.3	13.3	12.3	11.9	10.7	-1.2
Hallucinogens Adjusted ^d	NA	NA	NA	NA	18.0	15.7	15.7	15.0	14.7	13.3	-1.40
LSD	11.3	11.0	9.8	9.7	9.5	9.3	9.8	9.6	8.9	8.0	-0.9
PCP ^e	NA	NA	NA	NA	12.8	9.6	7.8	6.0	3.6	3.0	-0.6
Cocaine	9.0	9.7	10.8	12.0	15.4	15.7	16.5	16.0	16.2	16.1	-0.1
Heroin	2.2	1.8	1.8	1.6	1.1	1.1	1.1	1.2	1.2	1.3	+0.1
Other opiates ^e	9.0	9.6	10.3	9.9	10.1	9.8	10.1	9.6	9.4	9.7	+0.3
Stimulants ^e	22.3	22.6	23.0	22.9	24.2	26.4	32.2	33.6	35.4	NA	NA
Stimulants Adjusted ^{d,f}	NA	NA	NA	NA	NA	NA	NA	27.9	26.9	27.0	+1.0
Sedatives ^e	18.2	17.7	17.4	16.0	14.6	14.9	16.0	15.2	14.4	13.3	-1.1
Barbiturates ^e	16.9	16.2	15.6	13.7	11.8	11.0	11.3	10.3	9.9	9.9	0.0
Methaqualone ^e	8.1	7.8	8.5	7.9	8.3	9.5	10.6	10.7	10.1	8.3	-1.855
Tranquilizers ^e	17.0	16.8	18.0	17.0	16.3	15.2	14.7	14.0	13.3	12.4	-0.9
Alcohol	90.4	91.9	92.5	93.1	93.0	93.2	92.6	92.8	92.6	92.6	0.0
Cigarettes	73.6	73.4	75.7	75.3	74.0	71.0	71.0	70.1	70.6	69.7	-0.9

NOTES: Level of significance of difference between the two most recent classes:

* = .05, ** = .01, *** = .001.

NA indicates data not available.

^aData based on four questionnaire forms. N is four-fifths of N indicated.

^bAdjusted for underreporting of amyl and butyl nitrites (see text).

^cData based on a single questionnaire form. N is one-fifth of N indicated.

^dAdjusted for underreporting of PCP (see text).

^eOnly drug use which was not under a doctor's orders is included here.

^fAdjusted for overreporting of the non-prescription stimulants.

Other prevention approaches gained prominence in the 1970s. These programs focused on alternative activities to drug use. Some of these programs involved youth in community projects to reduce alienation, while others provided alternative opportunities for recreation, socialization, and informal education.

The effectiveness of affective education and alternative approaches to drug abuse has not as yet been demonstrated. While a significant number of programs have been evaluated (see for example Berberian et al. 1976; Goodstadt 1980; Schaps et al. 1981; Battjes 1985), the vast majority of studies have suffered from methodological and design flaws. It should be noted, however, that several of the studies indicate little or no effects for selected prevention approaches (Blum et al. 1978; Schaps et al. 1984).

PSYCHOSOCIAL PREVENTIVE INTERVENTION APPROACHES

Etiological research directed toward identifying and understanding the antecedents and correlates of drug abuse behavior has contributed significantly to the development of a new generation of preventive intervention approaches. The approaches that are addressed in this monograph have been variously titled "saying no" programs, peer resistance strategies, life skills training, and social inoculation. To maintain a consistent thread among a significant number of interventions reviewed in this monograph, the term social skills training will be applied to those preventive interventions that are broadly focused on enhancement of general personal and social competence skills (e.g., Botvin, Wills, Schinke, Pentz). Social inoculation training will be used for those programs that focus on skills training to resist peer and other social pressures (e.g., Johnson, Flay, Perry). The term cognitive-developmental training will be applied to those studies that focus on the physiological reaction to smoking experimentation (e.g., Leventhal and Glynn).

The chapters that follow address issues that have arisen as a consequence of current research experiences in the area of psychosocial prevention approaches to drug abuse among school-age children and adolescents. Botvin and Wills examine the results of "broad-spectrum" social skills approaches. Flay provides an analysis of the effectiveness of the social influences approach. Glynn, Leventhal, and Hirschman discuss a model of the natural history of cigarette smoking known as the cognitive-developmental approach. Biglan examines the methodological issues related to design and analysis of preventive intervention research strategies. Each of these papers is followed by a discussion chapter developed by Glasgow and McCaul, Lando, McCarthy, and Cook, respectively.

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Personal and Social Skills Training: Cognitive-Behavioral Approaches to Substance Abuse Prevention

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This chapter reviews the empirical evidence on the efficacy of substance abuse prevention efforts which teach generic personal and social skills. This type of substance abuse prevention strategy is part of a new generation of primary prevention programs which have proven effective in reducing the initiation of one or more forms of substance use (see Flay's chapter in this volume for a review of studies testing other recently developed substance abuse prevention strategies). The cognitive-behavioral approaches reviewed in this chapter are based on the postulate that the most effective approach to substance abuse prevention utilizing school-based programs with general population groups is to influence basic personal and social competence skills that appear to be causal factors for several different types of substance use and abuse.

This chapter begins with a review of the theoretical considerations that led researchers to focus on teaching students generic personal and social skills. Four current intervention models and reported results on the effectiveness of these approaches for preventing cigarette smoking and other forms of substance use are described. Particular attention is given to the process evaluations used in these studies, because one of the major contributions of these researchers has been to develop and adopt methods for assessing the personal and social competence factors that are hypothesized to be the mediators of program impact on substance use outcomes. The two final sections summarize the accomplishments of these programs, discuss methodological issues and potential limitations, and suggest some topics that seem likely to be productive for further research in this area.

BACKGROUND

Despite some awareness of the adverse consequences of substance use, a significant number of teenagers begin using psychoactive substances each year. Although for some persons use may be discontinued after a brief period of experimentation, initiation of tobacco, alcohol, or drug use may lead to patterns of use

which result in both psychological and physical dependence. In view of the difficulty and cost of treating individuals who have already developed substance abuse problems, the prospect of developing effective substance abuse prevention strategies has held a great deal of appeal over the years. While it seems evident that effective treatment programs will continue to be necessary, the development of effective prevention programs would clearly represent a major advance in the battle that has been waged against substance abuse for years.

Significant progress has been made in recent years which provides preliminary support for the efficacy of several new substance abuse prevention models. This new generation of programs differs from traditional prevention approaches by incorporating a more complete understanding of the basic causes of smoking, alcohol, and drug use/abuse. Perhaps more importantly, these programs have been subjected to carefully designed evaluation studies, which provide evidence of their effectiveness.

Psychological Inoculation Studies

The most significant breakthrough concerning substance abuse prevention first occurred in adolescent cigarette smoking. Evans and his colleagues at the University of Houston (Evans 1976; Evans et al. 1978) are credited with conducting the pioneering work in the development and testing of a strategy for countering social influences to smoke. In addition to using the social learning theory (Bandura 1977), Evans' work was strongly influenced by the persuasive communications theory of McGuire (1964, 1968). As such, a central feature of the prevention approach developed by Evans involved showing students films depicting the kinds of social pressures to smoke that they would likely encounter as they progressed through junior and senior high school. The main purpose of this approach was to "inoculate" students against such pressures. A further purpose of these films was to demonstrate specific tactics for resisting these pressures to smoke.

Other investigators have elaborated on this model. The work of McAlister and his colleagues (McAlister et al. 1979, 1980; Perry et al. 1980; Telch et al. 1982) and studies conducted in Minnesota (Arkin et al. 1981; Hurd et al. 1980; Luepker et al. in press; Murray et al. 1980), Canada (Flay et al. 1983), and Southern California (Flay et al. in press; Johnson et al. 1981) placed more emphasis on the actual training of students in how to resist social pressure to smoke. Results from studies testing this type of intervention, reviewed by Flay (this volume), show that this approach appears to be effective for reducing the incidence of new cigarette smoking in school-age populations.

Cognitive-Behavioral Programs

Other researchers in this area have examined prevention strategies having a somewhat broader focus. Instead of utilizing

a substance abuse prevention strategy that teaches knowledge and skills directly related to resisting offers of substance use (i.e., strategies which are specific to substance use), these strategies target general factors thought to be linked to subsequent substance use by teaching broad-based personal and social skills. Refusal skills and pressure resistance tactics are also taught as part of this strategy. Thus, skills and knowledge specific to substance abuse prevention are taught within the framework of programs designed to enhance general personal and social competence. This approach evolved from a theoretical model which posits that prevention can be achieved by teaching persons to deal more effectively with general life problems (e.g., interpersonal relationships, social coping, performance demands) and to cope effectively with specific temptations to use substances (Wills and Shiffman, in press). Although many of the techniques used in these programs were initially developed for remediation of existing deficits, recently these techniques have seen wide application in competence enhancement with normal populations (Pentz and Tolan submitted for publication).

THEORETICAL RATIONALE

Substance Use and Adolescent Development

The initiation of substance use is primarily an adolescent phenomenon, occurring within the context of great physical and psychological change. Adoption of one substance typically leads to experimentation with other psychoactive substances, and research with adolescents has shown a predictable sequence of initiations: individuals begin with tobacco and alcohol, progress later to marijuana, and may eventually go on to use other drugs such as depressants or opiates (Hamburg et al. 1975; Kandel 1978). For this reason, tobacco (nicotine) is frequently viewed as a "gateway" drug that greatly increases the probability of regular and/or problematic use of other substances. Psychological variables such as low self-esteem, depression, and poor coping skills are notably significant for predicting subsequent progression to problematic drug use (Kandel et al. 1978), and for this reason investigators have given extensive consideration to the psychological factors that are implicated in adolescent drug use.

During adolescence, individuals typically experiment with a wide range of behaviors and lifestyle patterns as part of the natural process of separating from parents, developing a sense of autonomy and independence, and acquiring some of the skills necessary for functioning effectively in the adult world. Profound cognitive changes occur during the beginning of adolescence which significantly alter the adolescent's view of the world and the manner in which he/she thinks. Also, due to what has been characterized as "adolescent egocentrism" (Elkind 1978), adolescents tend to have a heightened sense of self-consciousness concerning their appearance, personal qualities,

and abilities. Furthermore, as students approach adolescence, there appears to be a progressive decline in the impact of parental influence and a corresponding increase in the impact of influence from peer networks (Utech and Hoving 1969; Glynn 1981). This has profound consequences for substance use. Whereas parents and general adult networks tend to hold values that discourage substance use (at least by teens), peer networks may indirectly or directly encourage substance use (Wills and Vaughan 1984). Finally, in a process termed "adolescent invulnerability" (Urberg and Robbins 1983), teens perceive that they are not susceptible to the hazards presented by risk-taking and health-compromising behaviors.

These and other developmental changes occurring during this period increase adolescents' risk of yielding to various direct and indirect pressures to smoke, drink, or use drugs. The combination of adolescent egocentrism and the increased reliance on the peer group tend to promote substance use in some individuals. At the same time, the cognitive developments occurring prior to and during this period can serve to increase vulnerability to substance use influences by undermining knowledge related to the potential risk of using these substances. For example, adolescents' increased cognitive sophistication may enable them to discover inconsistencies or logical flaws in the arguments being advanced by adults concerning the potential risks of substance use, or may enable the adolescents to formulate their own counter-arguments and construct rationalizations for ignoring these risks—particularly if substance use is perceived as having social or personal benefits. Thus, a recognition of the developmental tasks, issues, changes, and pressures motivating adolescent behavior is necessary to fully understand the etiology of substance use/abuse.

Initiation and Early Stages of Substance Abuse

For most individuals, initial experimentation and the subsequent development of regular patterns of use typically occur during the pre-adolescent and adolescent period (Millman and Botvin 1983). The initial use of most substances tends to occur in social situations, with solitary use being relatively uncommon (Friedman et al. in press).

Evidence from a variety of sources suggests that social, attitudinal, and personality factors may all promote the initiation of substance use (Blum and Richards 1979; Braucht et al. 1973; Jessor 1976; Wechsler 1976). Social influences can originate from substance use by family members (particularly parents and older siblings) and friends, as well as from the portrayal of substance use in the popular media as something that is both acceptable and an important part of popularity, sex appeal, sophistication, success, and good times. On an individual level, a number of psychological factors have been associated with substance use. For example, some of the

psychological characteristics that have been associated with substance use/abuse include low self-esteem, a greater need for social approval, high anxiety, low assertiveness, an external locus of control, and an impatience to assume adult roles (Botvin and McAlister 1981; Millman and Botvin 1983).

Researchers have also found that substance users differ from nonusers along several behavioral dimensions, suggesting a difference with respect to value orientation. For example, individuals who use drugs tend to get lower grades in school, are less likely to participate in organized extracurricular activities such as sports or clubs, and are more likely than nonusers to engage in antisocial behaviors such as lying, stealing, and cheating (Demone 1973; Jessor et al. 1972; Wechsler and Thum 1973). Finally, evidence from a variety of sources indicates that certain substances tend to be used together; for example, adolescents who use opiates are also likely to drink excessively and to smoke.

As Jessor (1982) has noted, the association between several types of health-compromising behavior is perhaps one of the clearest facts to have emerged from the past decade of research. The significance of this observation is that a number of problem behaviors appear to be caused by the same underlying factors. For this reason, as well as for practical reasons, it has been suggested that prevention programs should be developed which target the underlying determinants of several theoretically and empirically related problem behaviors (Botvin 1982; Swisher 1979). This postulate is the basis for the generic substance abuse prevention programs, which aim at increasing students' general personal and social competence, thereby affecting the factors that underlie many types of substance use and health-compromising behavior.

Theoretical Foundations

All of the newer psychosocial prevention strategies have common theoretical roots based largely on social learning theory (Bandura 1977) and problem behavior theory (Jessor and Jessor 1977). From this perspective, substance use is conceptualized as a socially learned, purposive, and functional behavior which is the result of the interplay of social-environmental and personal factors. Substance use behavior, like other types of behavior, is learned through a process of modeling and reinforcement which is mediated by personal factors such as cognitions, attitudes, and beliefs.

This modeling and reinforcement process can occur in several ways. Some individuals may seek out other individuals who smoke, drink, or use drugs—or may be motivated to engage in those behaviors themselves—as a way of coping with expected failure or as an alternative way of achieving some desired goal (i.e., some adolescents who are not doing well academically may begin to use drugs as an alternative means of achieving popularity, social

status, or self-esteem). Similarly, the use of tobacco, alcohol, and certain other drugs may be used in an attempt to cope with tension or anxiety, particularly social anxiety.

Other individuals may begin smoking, drinking, or using drugs after repeatedly observing high status role models engaging in these behaviors or as the result of persuasive appeals made by advertisers or peers. Differential susceptibility to social influence appears to be mediated by personality—with individuals who have low self-esteem, low self-confidence, low autonomy, and an external locus of control being more likely to succumb to these influences (Bandura 1969; Rotter 1972).

An argument, supporting broader-based prevention programs, is that successful prevention strategies must not only provide students with the skills necessary to resist social pressure to use substances, but must also reduce potential motivations to engage in tobacco, alcohol, or drug use. The former might be accomplished by increasing students' refusal skills. The latter might be accomplished by improving students' general personal and social competence. A potential limitation of pressure-resistance prevention models is that, although students might learn refusal skills, they might have little motivation to apply these skills. In fact, Friedman et al. (1983), in an empirical study of students who began smoking in social situations, found that the majority had knowingly entered the situation with the specific intent of smoking. For these reasons, cognitive-behavioral prevention models combine training in the use of refusal skills with interventions intended both to improve general coping skills and indirectly modify psychological factors related to substance use.

Personal and Social Competence

The acquisition of adequate social skills appears to play an important role in both psychological adjustment and psychosocial development. Basic interpersonal skills are necessary for confident, responsive, and mutually beneficial relationships, and are perhaps among the most important skills that an individual must learn. A lack of social competence may lead to rejection and social isolation, which may in turn result in poor psychological adjustment.

Individuals generally begin the acquisition of basic social skills during childhood, and as they mature their social skills generally increase. By the time individuals become adolescents, many have acquired a repertoire of social skills such as initiating and maintaining conversations, communicating effectively, giving and receiving compliments, refusing unreasonable requests, expressing feelings, etc. Social skills, like other behaviors, are learned through a combination of modeling and reinforcement and the development of these skills is dependent upon having the opportunity to observe and practice them. In addition to acquiring general social skills, it is

important that adolescents learn the kind of refusal skills that will enable them to resist peer pressure to engage in behaviors that they might otherwise choose to avoid.

Social skills may represent the largest collection of skills which individuals need to develop in order to function effectively as adults. However, it is also necessary to acquire personal skills relating to effective and responsible decision making, techniques for coping with stress and anxiety, and basic principles of personal behavior change and self improvement.

Information and Knowledge

Available evidence indicates that teaching only about the extreme negative consequences of substance abuse is of marginal value as a prevention strategy. This does not mean that information and knowledge cannot play an important role in substance abuse prevention. Certain types of knowledge about the use of tobacco, alcohol, and drugs may be a useful component of substance abuse prevention programs. For example, since adolescents typically overestimate the prevalence of smoking and drinking, correcting normative expectations of high substance use might help to reduce the perceived social support for these behaviors.

Furthermore, information and knowledge included in prevention programs should be selected with an understanding of cognitive-developmental factors. Since adolescents tend to have a "present-time" orientation, the perceived short-term social benefits of smoking, drinking, or drug taking may override concerns for potential negative consequences (particularly more distant long-term ones). Consequently, focusing on some of the more immediate consequences of use, which may serve as social liabilities (e.g., nicotine stains on teeth, bad breath, etc.), might be a somewhat more meaningful deterrent than providing information on long-term consequences.

Another important issue concerning information and knowledge is its perceived credibility. Factual information about the potential hazards of drug use, for example, may be ignored when presented by teachers, but taken seriously when presented by peer leaders. Moreover, the manner in which information is presented can either enhance or detract from its credibility. For example, information presented in an obviously one-sided and biased manner is likely to be viewed with more suspicion than information that is presented in a balanced and neutral manner.

INTERVENTION STRATEGIES AND EVALUATION RESULTS

Research on generic personal and social skills training approaches to substance abuse prevention has been conducted primarily by four research groups around the country during the past few years. This includes research by Pentz (initially at the University of Tennessee and more recently at the University of Southern California), research by Schinke and his colleagues

(at the University of Washington), research by Wills (initially at the American Health Foundation and more recently at Cornell University), and research by Botvin (at Cornell University).

While each of these research groups developed substance abuse prevention programs independently, there are many existing commonalities in terms of both method and rationale. However, a number of important differences exist within the approaches utilized by these research groups. Moreover, in spite of the fact that all of these intervention approaches have similar roots, the case for each type of intervention strategy is argued somewhat differently. A discussion of the work conducted by these four research groups follows. Each section contains a summary of the rationale used for that particular intervention strategy, a general description of the intervention program, and a summary of the results of evaluation studies conducted with each intervention strategy.

Social Assertiveness Skills Training (Pentz)

The research being conducted by Pentz (1983, in press) tests a prevention approach that is based on a social competence model of substance use. Substance use in early adolescence, according to this model, is a joint product of social influences (e.g., parental smoking, peer smoking) and poor social assertiveness skills that are relevant for dealing with situations in which individuals may experience social pressure to smoke, drink, or use drugs. Pentz's model is derived from social learning theory (Bandura 1977) and problem behavior theory (Jessor and Jessor 1977), and is based on several empirical findings. First, a number of studies have found substantial correlations between self, peer, and parental substance use. Second, other researchers have reported associations between substance use and such variables as high anxiety, depression, low self-esteem, and low self-efficacy, suggesting that students who are prone to substance use are relatively low in social competence. Third, problem behaviors such as school failure, delinquency, and aggression have been found to be correlated with substance use.

According to Pentz (1983), these findings suggest that students with low social competence may be more susceptible to frequent use of drugs or alcohol as a means of relieving social anxiety. It is also hypothesized that students with low social competence are less able to deal with various kinds of explicit social pressure to smoke, drink, or use drugs. Adolescents who experience a delay in the development of social competence are viewed by Pentz as being prone to delinquency, aggressiveness, academic or social withdrawal, and substance use.

The intervention approach developed by Pentz (1983) is, therefore, based on the assumption that the initiation of substance use can be deterred by increasing social competence and self-efficacy. Social competence is essentially conceptualized as assertiveness (i.e., the ability to disagree, to refuse, to make

requests, and to initiate conversations). Self-efficacy is defined as "the conviction that one can successfully execute behaviors necessary to produce desired outcomes" (Pentz, in press). It is assumed that in early adolescence, intentions to experiment with drugs may be offset by the development of self-efficacy and prosocial skills.

The content and techniques incorporated into the prevention program are derived from cognitive-behavior therapy and, more specifically, from assertiveness skills training (see Pentz 1983). The intervention program consists of seven 55-minute sessions which are conducted by a trained teacher paired with a program assistant, and facilitated by peers working in subgroups of four in the classroom. Skills for dealing with several social situations are demonstrated and taught through modeling, rehearsal, and feedback to participating students. The social situations covered include everyday situations with teachers, parents, and peers.

One evaluation study of this approach has been conducted so far (Pentz, in press). Evaluation measures included a set of self-report items on recent use of cigarettes and alcohol, a self-efficacy scale, and a role-play measure of social competence. The smoking and drinking self-report items are scored for frequency of use in the past month, amount of use and number of days used in past month, and number of times abused in the last 6 months. The self-efficacy scale is composed of 18 items which tap perceived efficacy in both familiar and unfamiliar interpersonal situations with teachers, parents, and peers. The role-play measure of social competence involves presenting students with 12 audiotaped social situations to which they must respond. Responses are audiotaped and subsequently coded on a 5-point scale of social competence.

Also included was a teacher rating scale in which students were rated by classroom teachers on a 10-point scale ranging from very unassertive to very aggressive, with the midpoint representing appropriate assertive behavior. Students were classified into three groups (unassertive, assertive, aggressive) on the basis of these teacher ratings. Several items relevant to school behavior (e.g., semester grade point average, truancy, absence) were derived from students' self-reports and from school records. In addition, a 50-item inventory of attitudes towards substance use was completed by students.

The evaluation study involved 1,193 students from 8 schools in rural and suburban Tennessee, ranging from 6th through 9th grades. Classrooms were randomly assigned to one of three experimental conditions: full intervention (seven sessions of instruction, modeling, and behavioral rehearsal), partial intervention (two sessions of instruction only), or full control. Half of the schools involved in the study were in rural areas and half were in suburban areas.

Outcome results indicated several significant findings. The intervention program increased social competence (as indexed by the role-play measure), self-efficacy (as indexed by the questionnaire measure), and grade point average. This effect was more profound in the full intervention group, in comparison to the instruction-only or control group. An interaction effect was found between social skills training and initial behavioral style—the intervention program had its greatest effect on those students who were assertive or aggressive at baseline. These students showed the largest increases in social competence and improvements in student-teacher relationships, family cohesiveness, and rebelliousness.

A similar interaction effect was also found for substance use. Students in the full intervention group who had been assertive or aggressive at baseline showed the greatest decrease in onset rates for substance use, compared with either the instruction-only or the control group. Results for social competence measures were maintained over all follow up measurements in this study; results for drug use measures decreased somewhat over time, so that by the 6 month follow up the full intervention and instruction-only groups showed similar levels of drug use. Data presented by Pentz (in press) indicate that the effect of the intervention occurred primarily for alcohol use (beer, wine, and hard liquor); no effects were noted for cigarette smoking.

Interactions with age of intervention suggested that the intervention program had its greatest effect on self-efficacy, social competence, and drug use for the 6th and 9th grade students. It is noteworthy that these represent crucial transition periods for adolescents. These intervention effects were maintained through the 6 month follow up. Interactions with rural versus suburban residence suggested that in rural areas drug use was related primarily to social competence; whereas in suburban areas, substance use was affected by a broader range of variables, including drug use influences (e.g., higher peer and parental drug use, more negative social attitudes).

In sum, social competence training, in interaction with grade level and personality style (aggressive versus nonassertive), produced increases in social competence and school grades, and decreases in drug use. To a lesser extent, environmental characteristics appeared to moderate program effects, so that social competence training had a greater effect on drug use among rural students compared with suburban students. The results also indicated that the intervention program was most effective for drug abuse prevention when conducted just before periods of crucial transition for adolescents (e.g., beginning of junior high school or beginning of high school).

The results reported by Pentz show some potential benefits of her intervention program as a substance abuse prevention strategy, although these benefits appear to be modest. However,

it is puzzling that no effects were obtained for cigarette smoking, in contrast to results from most other studies; possibly this is a consequence of the population characteristics of students in rural/suburban Tennessee, where the study was conducted. Perhaps more informative was Pentz's analyses of the etiological determinants of substance use (conducted with control group data). The results of these analyses (which have not been included here) generally appear to support her hypothesized causal model of substance use, and also were consistent with the theoretical rationale for the intervention program.

Cognitive-Behavioral Skills Training (Schinke)

Schinke and his colleagues at the University of Washington have conducted a series of studies examining the extent to which a cognitive-behavioral intervention program is capable of preventing or reducing cigarette smoking among adolescents. The intervention strategy developed and tested by Schinke and his colleagues is derived from their extensive research in the area of pregnancy prevention (Gilchrist and Schinke, in press; Schinke 1982; Schinke et al. 1980; Schinke and Gilchrist 1977). Schinke and colleagues view cigarette smoking, like engaging in sexual intercourse without the use of contraceptives, from both a developmental and social learning theory perspective.

Adolescence is a time for acquiring adult skills, exploring options, and taking risks. Instead of letting adolescents flounder through the adolescent years, learning skills in a chaotic, hit-or-miss fashion, it is argued that adolescents should be deliberately and systematically taught the skills necessary to enjoy happy, healthy, and prosperous lives (Schinke and Gilchrist 1984). The cognitive-behavioral approach is designed to enable adolescents to acquire both the personal and social competence skills necessary for them to "handle current problems, anticipate and prevent future ones, and advance their mental health, social functioning, economic welfare, and physical well-being" (Schinke and Gilchrist 1984).

Schinke's approach recognizes the fact that adolescents are frequently influenced or pressured into participating in peer-sanctioned behaviors such as smoking, drinking, and sexual intercourse. Although adolescents may possess knowledge concerning the health risks associated with engaging in these activities, the perceived social benefits of engaging in these behaviors may override this knowledge. Adolescents who have not acquired appropriate cognitive and behavioral skills not only are unable to make well-informed and well-reasoned decisions concerning such situations, but also will be likely to lack the ability to successfully implement a decision that goes against group norms.

The general approach advocated by Schinke and his colleagues involves the acquisition of decision-making and problem-solving skills which might enable adolescents to more fully utilize the

information at their disposal and be better prepared to make decisions involving health-compromising behaviors. In addition, adolescents are taught interpersonal skills so that they can communicate effectively and assertively.

This approach emphasizes the development of cognitive and behavioral skills which are flexible and not situation-specific. On a cognitive level, students are taught decision-making and problem-solving techniques which will better prepare them to avoid peer pressure situations without alienating friends. Students are also taught specific self-instruction techniques that are designed to provide them with a framework for guiding themselves through high-risk situations. Finally, students are taught basic interpersonal skills designed to enable them to implement specific decisions or act in a way which is consistent with what they want.

The main components of this intervention strategy include: (1) providing accurate information which can be immediately personalized; (2) teaching students how to handle a diverse range of problem situations through a systematic stepwise problem solving strategy, as well as techniques such as brainstorming which can help individuals develop potential solutions; (3) conveying self-instructional techniques designed to help individuals exercise self-control over their behavior (i.e., the verbal mediation of behavior through covert instruction); (4) teaching adaptive coping strategies for relieving stress, anxiety, and pressure involving both covert cognitive coping skills and overt relaxation training techniques; and (5) developing important verbal and nonverbal assertive skills. Classes or small groups provide opportunities for modeling, reinforcement, feedback, and the vicarious learning of important personal and social skills.

Adult group leaders teach students systematic decision-making procedures for handling difficult situations. Case examples from personal experiences of students are used to master the various steps of defining a problem clearly, generating possible solutions, selecting one solution, and planning its implementation. Students are also taught important assertive communication skills through role plays designed to provide them with practice in sticking to tough decisions, dealing with risky situations and influential people, and using self-control. A combination of modeling, feedback, reinforcement, and coaching is utilized to teach these skills. Homework assignments are employed to give students additional practice and to increase the likelihood that these decision-making skills will be utilized in various life situations.

To date, Schinke and his colleagues have completed three studies applying this general prevention model to adolescent cigarette smoking. The cognitive-behavioral intervention strategy used in these studies was essentially the same as the one specified above. However, it does not appear to have included the stress management component.

The first of these studies (Schinke and Blythe 1981) was conducted with 6th graders ($N = 28$) from two classes within the same school. Students were randomly assigned to experimental and control groups. All students were pre- and posttested by questionnaire for smoking knowledge and three measures related to problem solving and decision making (perspective taking, means/end thinking, and anticipation of consequences). In addition, students were videotaped in order to assess specific interpersonal (assertive) skills in eight face-to-face interactions involving saying "no" to offers of cigarettes, extolling the wisdom of not smoking, and pointing out the hazards of tobacco use. A 6-month follow up was conducted in which enhanced self-reports of weekly and monthly cigarette smoking were collected using the bogus pipeline technique; students also completed questionnaires designed to measure their attitudes toward nonsmoking, intentions to smoke in the future, and the number of refusals to smoke when offered.

The cognitive-behavioral intervention program consisted of eight 60-minute sessions conducted in small groups. The program provided students with: general health information concerning the advantages and disadvantages of smoking; problem solving and decision making skills; and cognitive-behavioral strategies for resisting peer pressure to smoke. Students were also taught self-instruction techniques to help them exercise control over their own behavior and set and achieve specific goals (particularly goals related to resisting peer pressure to smoke). Finally, students were taught assertive communication skills (e.g., maintaining eye contact, appropriate facial expressions, hand gestures, loudness of voice, assertive statements). All of these skills were taught using a combination of modeling, role playing, feedback, and reinforcement.

The intervention program was conducted by graduate interns who provided students with feedback, reinforcement, and coaching. Additionally, students took turns in different roles and as coaches. Extended practice in the form of behavioral homework assignments was also part of the program (e.g., gathering antismoking information, observing cigarette smoking in public places, leaving situations where others were smoking, and asking people not to smoke).

Two-tailed t-tests were used to compare the treatment and control groups at the posttest in terms of gain scores for smoking knowledge, problem solving and decision making, and videotaped social interaction measures. Both groups were also compared by t-test for the various follow-up measures. Results yielded significant differences between the two groups, indicating that the students in the treatment group had significantly greater increases in smoking knowledge, problem solving and decision making skills, and assertive skills (higher rates of eye contact, greater use of "I" and "no," and more requests that peers not smoke).

Six-month follow up data indicated significantly less cigarette smoking for the prevention condition, both in terms of cigarettes smoked in the past week and past month. In addition, the students in the prevention condition had more positive attitudes toward nonsmoking, fewer intentions to smoke, and more refusals of cigarettes. Follow up smoking data were collected using the "bogus-pipeline" procedure. Although not explicitly stated, it appears that conventional self-report procedures were used to collect pre- and posttest smoking data. No pre- or posttest data are presented for any of the variables reported at the 6-month follow up. However, the authors state that "between condition analyses of pretest data on young peoples' self reported cigarette use and their reports of family members' smoking were nonsignificant" (p. 34).

A second study (Schinke and Gilchrist 1983) was conducted with 6th grade students (N = 56) from two schools who were randomly assigned to four conditions of a Solomon (1949) four-group research design: pretest and intervention, intervention only, pretest only, and neither the pretest nor the intervention program. All students were posttested after 2 months, and follow up data were collected after 6 months. As in the previous study, the intervention program consisted of eight 1-hour sessions conducted twice a week by two group leaders. The intervention program was essentially the same as in the first study, involving a combination of information, the acquisition of decision-making and problem-solving skills, self-instruction and self-management skills, and assertiveness skills training.

The intervention material was delivered through the use of audiovisual aids, handouts, class discussion, modeling, behavior rehearsal, and extended practice. With respect to the latter, students participated in outside homework assignments in which they were asked to gather additional information about cigarette smoking, observe cigarette smoking in public places, leave places where other individuals were smoking, refuse to smoke, ask peers not to smoke, and proselytize the advantages of not smoking to classmates and adults.

The same type of multiple assessment measures used in the previous study (Schinke and Blythe 1981) were used in this study to compare the various treatment and control groups. Data were analyzed using analyses of covariance, with pretest scores being used as covariates. Once again, at the posttest students participating in the intervention program were found to be more knowledgeable about smoking hazards, were more skilled in decision making and problem solving, and their performance on videotaped interactions indicated a greater degree of assertiveness.

No data were presented concerning smoking behavior either at the pre- or posttest. At the 6-month follow up, the students who

participated in the prevention program reported more instances of refusing cigarettes and fewer intentions of ever smoking. Enhanced self-report data indicated significantly less tobacco use among the students participating in the prevention program. In terms of the proportion of students smoking, 8% of the students in the prevention program and 37.5% of the control students reported smoking in the past month.

A third and larger study (Schinke and Gilchrist, in press) examined the effectiveness of this type of prevention strategy with 234 middle and lower middle class white students in 6th grade. All students were pretested and randomly assigned by classroom to the following conditions: cognitive-behavioral skills building, attitude modification, and control. Group leaders were pairs of social workers randomly assigned to each of the two treatment conditions.

As in the previous studies, students in the prevention condition participated in eight 1-hour class sessions, which provided training in the use of self-management skills, problem-solving and decision-making skills, and interpersonal (assertive) skills. To facilitate learning assertiveness skills, students watched videotapes of peers who avoided smoking under difficult circumstances. Students subsequently discussed the use of specific skills and practiced them in role-play situations.

One interesting difference between this study and the previous ones was that the students assigned to the attitude modification condition were provided with the same health information about cigarette smoking as the students participating in the cognitive-behavioral program. In addition, the students in the attitude modification condition learned the merits of nonsmoking through oral quizzes and contests modeled after TV game shows, participated in anti-smoking skits, and made public commitments not to smoke.

Students were assessed on the same kind of mediating variables included in the two previous studies. Saliva samples were collected along with enhanced self-reports of smoking behavior. Data were analyzed by means of analyses of covariance, with pretest scores being used as covariates for the analysis of posttest and follow up data. Results indicated that in the skills training and attitude modification conditions, students were more knowledgeable about smoking effects than the control students. The skills training group scored better on problem-solving and decision-making skills than either group, and performed better on all measures of assertive communication skills. Cigarette smoking from posttest to 6-month follow up and from 6-month follow up to 12-month follow up for the skills building condition were significantly lower than for the attitude modification condition and than for the control condition. Comparison of saliva thiocyanate (SCN) levels indicated that the three conditions did not differ over any measurement period.

In summary, the work by Schinke and his colleagues has consistently demonstrated the effectiveness of an eight session cognitive-behavioral approach to smoking prevention when conducted with outside professionals. Moreover, this approach has produced significant increases in several measures of problem-solving and decision-making skills, smoking knowledge and attitudes, several in vivo measures of interpersonal assertive skills, and intentions to smoke in the future. Although these studies have generally involved relatively small sample sizes, they have nonetheless included random assignment of individuals (in two studies) and classes (in one). However, lack of sufficient detail in the research reports of these studies concerning the measures used to assess cigarette smoking make comparison with other prevention studies difficult.

Decision Skills Curriculum (Wills)

Wills (in press) has been conducting a program of research that has not only tested the effectiveness of a substance abuse prevention program, but has also examined the relationship among stress, coping, and substance use in adolescents. While it is recognized that substance use initiation is a multifactorial process that involves availability of substances, substance-related knowledge and attitudes, and social influences from parents and peers (Bentler and Speckart 1979; Botvin and McAlister 1981), a major focus of Wills' research has been on the psychosocial stress factors that may predispose adolescents to begin using substances such as tobacco and alcohol. This focus was suggested in part by a body of research, derived largely from studies of adults (see Leventhal and Cleary 1980), suggesting that smoking serves a direct stress-reduction function. Another focus of Wills' research has been on the measurement of coping patterns relevant for substance use in early adolescence. Epidemiological research (e.g., Kandel 1978) has suggested that poor coping in several life domains (e.g., parental relationships, school performance, coping with negative emotions) is a common underlying factor in the predictive patterns observed for many different types of substance use.

Wills has utilized several different measures of stress and coping in both his etiologic and intervention research. Questionnaires were used to assess subjective symptoms of stress (e.g., tension, difficulty in relaxing), occurrence of recent events (during the previous week) that could evoke stress, and occurrence of major life events during the past year that could objectively be classified as stressful. Measurement of coping was based on a coping inventory (Bugen and Hawkins 1981) that asked subjects to indicate their frequency of doing various things when they had a problem at school or at home. Factorial analyses of this generalized coping measure indicated major dimensions termed decision-making (which reflected an active, problem-solving approach to coping) and cognitive coping (which represented a more cognitive or emotion-focused approach to dealing with problems). Other coping factors reflected social

support from peers and from parents, involvement in entertaining or diverting activities, physical exercise, and meditation or prayer. Also used in this research were several relevant psychological variables including health locus of control (Wallston et al. 1978), assertiveness, and generalized self-regard.

Multivariate analyses of data from several cohorts of inner-city 7th graders indicated that stress measured by all three methods significantly increased the probability of involvement in substance use (both smoking and alcohol), as did external locus of control and low self-esteem. The use of both decision-making and cognitive coping methods significantly decreased the probability of substance use. General involvement in entertainment activities and peer networks was positively associated with substance use, whereas social support from parents and other adults was negatively associated with substance use. Because Wills found that these psychosocial variables were significant correlates of substance use among adolescents, he included them as process variables in his intervention research to examine their role as mediators of the impact of his substance abuse prevention program.

Based on a psychosocial model of substance abuse described elsewhere (Wills and Shiffman, in press), a substance abuse prevention program termed the Decision Skills Curriculum (Spitzhoff et al. 1981) was developed. The intervention program was designed to affect mediating variables relevant for deterrence of smoking initiation (i.e., decision-making ability, locus of control, knowledge about negative consequences of smoking, and assertiveness skills).

The prevention curriculum consisted of eight modules, and was taught in the first project year in consecutive sessions over a 2-week period. The curriculum was taught by project staff (two health educators), while the regular classroom teachers observed and assisted where necessary with exercises and activities. The curriculum began with a values-clarification module which included a values-clarification exercise focused on leisure activities. The next two modules contained material on decision-making. First, decision making was introduced as a systematic process for dealing with general problems of adolescence. Next, the decision-making skills taught were applied to everyday decisions and to substance-related decisions in role-play exercises using prepared scenarios.

Following these modules were two on social influence, applying this concept both to general influences (especially media advertising about smoking) and to specific influences such as peer pressure for substance use. In the first of these modules, instruction was given on counteracting adverse social influences, using both cognitive and direct-action approaches. Following from the latter concept was a module on assertiveness, which began by introducing the distinction between aggressive,

assertive, and passive behavior. This concept was then applied in role-play exercises involving both general assertiveness situations (e.g., being served in a department store) and substance use situations (e.g., being offered a cigarette in a group setting).

Two modules on stress management were also included which dealt with both short- and long-term issues. The first module taught an approach for dealing with stressful situations based on the cognitive modification approach of Meichenbaum (1977). This approach uses a four-step process of preparing for and coping constructively with stressful situations such as test-taking or new social encounters. The other module presented ways of incorporating stress-management techniques into one's lifestyle, focusing on progressive muscle relaxation. Other activities such as meditation and physical activity (e.g., running) were discussed as positive ways of using leisure time and dealing with periods of stress.

A final module on the health consequences of smoking provided cognitive material on both short- and long-term physiological effects of cigarette smoking. This module included a lecture section, a biofeedback demonstration on the physiological effects of nicotine (using pulse rate, blood pressure, and hand tremor), and a discussion of the psychological and economic benefits of non-smoking.

In addition to the curriculum, a health screening examination was conducted in the treatment schools by auxiliary medical personnel. This examination was designed to increase general health awareness and sense of personal responsibility for health. The procedure included measurements of height and weight (with computation of a relative weight index), body fat (measured by the skinfold thickness of triceps), blood pressure, and cardiovascular fitness (measured by the Harvard Step Test). Students participated voluntarily in the examination and received feedback on their relative health status through comparison with normative data from national health statistics. The activity was presented as a positive experience, wherein students could receive individualized information about themselves.

The intervention program was conducted with the entire seventh grade (N = 800) in three New York City junior high schools. Two of the schools (School E1 and School E2) were assigned to the experimental condition, receiving the Decision Skills Curriculum and associated educational activities; the other school was assigned to the control condition. Baseline data indicated that the schools were closely matched on rates of regular smoking.

The intervention program consisting of eight sessions was first implemented immediately after completion of the baseline survey in Fall 1981. A follow up program, which was based on the original curriculum and composed of six sessions, was implemented with the same population during their 8th grade year. Evaluation

data were obtained by a questionnaire which was administered in school classrooms by project staff at the beginning and end of each school year. The questionnaire included measures of smoking and alcohol use as well as the measures of stress, coping, and locus of control discussed above. Self-report data for cigarette smoking were collected using the bogus pipeline procedure.

Preliminary analyses of data on substance use and process variables indicated that the overall effect for the intervention was moderated by school differences. Thus, further analyses were performed at the school level, contrasting each of the two experimental schools with the control school. It is evident from the data on smoking incidence that the intervention was effective in school E1. In this school, there was a 42% reduction in new experimental smoking at the end of 7th grade (approximately four months after the conclusion of the intervention) and a 39% reduction in regular experimental smoking at the beginning of the 8th grade (10 months after the conclusion of the intervention). For school E2, however, there was no significant effect on smoking, even though the identical intervention had been implemented in both of the experimental schools. Data for alcohol use (defined by the heavy drinking measure) were mixed, showing a nonsignificant reduction in school E1 and an increase in school E2.

Data on the process variables were consistent with the outcome data on substance use. The process data were analyzed through analysis of covariance, with the baseline measure as the covariate. Data collected at the end of the 7th grade indicated that in school E1 there were significant increases in decision-making skills and internal health locus of control, and significant decreases in stress and in substance use as a coping strategy. Other changes for cognitive coping, social support, and substance-related assertiveness were in the same direction but were nonsignificant. Data collected at the beginning of the 8th grade (10 months after the conclusion of the intervention) indicated that changes in the coping patterns along the dimensions of decision making, cognitive coping, and social support, as well as for internal health locus of control, were maintained over time in school E1. In contrast, no significant desired changes were noted for school E2. Indeed, some changes in a nondesirable direction (increased stress and tendency to use substances as a coping strategy) were found for this school.

These data indicate that the intervention program was differentially effective in the two experimental schools. In one experimental school, it affected the process variables in the predicted direction and produced a significant reduction in smoking initiation, while it was not effective in the other experimental school. Because no systematic data on school atmosphere were collected, interpretation of these results is somewhat speculative. However, anecdotal evidence obtained from the project staff suggest the presence of several salient characteristics which distinguished the two treatment schools

from one another, and may have accounted for the observed differences in outcome results. These include the level of support and cooperation by the principal, school discipline, and the relationship between teachers and administrators.

The results of this study illustrate the necessity of having a number of different schools in any given study. Results may be markedly affected by the atmosphere of individual schools, which can vary greatly even within an apparently homogenous population. It is noteworthy that in Wills' study the schools were matched on rates of regular smoking at baseline so that differential results cannot be attributed to differences in baseline smoking.

Life Skills Training (Botvin)

Research by Botvin and his colleagues has involved the development and testing of a broad-spectrum prevention strategy called Life Skills Training (LST). The main purpose of this approach is to facilitate the development of generic life/coping skills as well as skills and knowledge more specifically related to resisting social influences to smoke, drink, or use drugs. A central feature of the LST program is the teaching of several cognitive-behavioral skills found to be effective when used to remediate psychological or behavioral deficits. However, within the context of this program, all students are taught these skills in order to enhance their ability to, for example, cope more effectively with anxiety or to function more competently in social situations.

Some of the general cognitive-behavioral techniques incorporated into the LST Program include cognitive strategies for enhancing self-esteem (e.g., goal setting, behavior change techniques, increasing positive self statements); techniques for resisting persuasive appeals (e.g., identifying persuasive appeals, formulating counter-arguments); techniques for coping with anxiety (e.g., relaxation training, mental rehearsal); verbal and nonverbal communication skills; and a variety of social skills (e.g., initiating social interactions, conversational skills, heterosocial ("dating") skills, complimenting, verbal and nonverbal assertive skills). These skills are taught using a combination of instruction, modeling, rehearsal, feedback and reinforcement, and extended practice through homework assignments.

In addition to providing students with general life skills, this prevention strategy also involves teaching students skills and knowledge more specifically related to the problem of substance abuse. For example, in addition to teaching students general assertive skills (i.e., the use of "no" statements, requests, and the assertive expression of rights), students are taught how to use these skills to resist direct interpersonal pressure to smoke. Thus, students are not only taught a wide range of personal and social skills in order to improve their general competence and reduce potential motivations for substance use,

but are also taught how to apply these skills in situations in which they may experience social pressure to smoke, drink, or use drugs.

The LST program is composed of three major components. The first component (Substance-Specific Component) contains material similar to that contained in many of the newer psychosocial smoking prevention programs (e.g., Evans et al. 1978; Hurd et al. 1980; McAlister et al. 1979). Elements of this component include: material describing the short- and long-term consequences of substance abuse; information about prevalence rates among both adults and adolescents in order to correct normative expectations; information and class exercises demonstrating the immediate physiological effects of cigarette smoking using biofeedback-type apparatus; material concerning media pressures to smoke or drink; and techniques for resisting direct peer pressure to smoke, drink, or take drugs.

The second component (Personal Skills Component) contains material: concerning decision making that has been designed to foster the development of critical thinking and responsible decision-making; designed to provide students with techniques for coping with anxiety (i.e., cognitive and behavioral self control strategies); and designed to provide students with the basic principles of personal behavior change and self improvement.

The third component (Social Skills Component) contains material designed to improve general interpersonal skills. Elements of this component include: material concerning effective communications, general social skills (e.g., initiating social interactions, conversational skills, complimenting); skills related to male/female relationships among adolescents; and both verbal and nonverbal assertive skills.

The initial pilot research with this psychosocial prevention strategy (Botvin et al. 1980) tested its short-term effectiveness for preventing the onset of cigarette smoking when implemented by members of the project staff. This study was conducted with 8th, 9th, and 10th graders (N = 281) from two different schools in suburban New York. Both schools were comparable with respect to socioeconomic status (SES) and baseline smoking rates. Schools were randomly assigned to experimental and control conditions. One school participated in a 10 session prevention program (LST) and the other served as a no-contact control group. All students were pretested and posttested by questionnaire with respect to self-reported smoking status, knowledge about cigarette smoking (e.g., the immediate effects of cigarette smoking, smoking prevalence among adults and adolescents), psychosocial knowledge, locus of control, self-esteem, social anxiety, decision making autonomy, and the need for group acceptance. Three months after the first posttest, data were once again collected concerning self-reported smoking status.

The results of this study indicated that significantly fewer

students in the experimental group (4%) began smoking (one or more cigarettes per month) than in the control group (16%) at the time of the initial posttest. The two groups were also compared in terms of the hypothesized mediating variables mentioned above. Both groups were compared by means of a two-way analysis of variance (sex X treatment condition) conducted using change scores. Significantly greater increases were found for the treatment group with respect to smoking knowledge, and significantly greater decreases were found with respect to need for group acceptance and social anxiety (for males only). All of these changes were in a direction consistent with nonsmoking.

Follow up data collected three months after the initial posttest (Botvin and Eng 1980) indicated that there still were fewer students in the experimental group beginning to smoke than in the control group (6% vs. 18%). Overall, the LST prevention strategy produced a 75% reduction in new cigarette smoking over the three months between the pretest and posttest which decreased over the three months between the posttest and the follow-up to a 67% reduction. Although this pilot study was extremely encouraging, interpretation of these results is limited by the fact that conventional self-report data were used (i.e., self-report data were not collected using the bogus pipeline procedure) leaving open the possibility that these results may have been biased by under-reporting.

The second study (Botvin and Eng 1982) with this approach involved testing the efficacy of the LST prevention program when implemented by older (11th and 12th grade) peer leaders. The program was tested on 7th graders (N = 426) from two public junior high schools in suburban New York City. Schools were randomly assigned to experimental and control conditions. Furthermore, saliva samples were collected prior to administration of the self-report questionnaire using the "bogus pipeline" procedure to enhance the quality of the self-report data (Evans et al. 1977) and to provide an objective measure of smoking status (saliva thiocyanate). In addition to data on smoking behavior, students were assessed in terms of their smoking knowledge, advertising knowledge (i.e. knowledge concerning how advertisers attempt to influence consumer behavior), locus of control, self-esteem, social anxiety, and decision-making autonomy.

Once again the results indicated that there were significantly fewer new smokers in the experimental group (8%) than in the control group (19%). These results were corroborated by the results of the saliva thiocyanate (SCN) analysis, conducted on a 30% subsample. These analyses showed a significant increase in SCN levels (indicating increased smoking) for the students in the control group and no significant increase for the students in the experimental group. The cognitive and personality variables were analyzed by means of a one-way analysis of covariance, with pretest scores being used as covariates. Significant differences consistent with nonsmoking were found between the experimental

and control groups for smoking knowledge, psychosocial knowledge, advertising knowledge, social anxiety, and decision-making autonomy. Discriminant function analyses were computed to determine the extent to which new smokers could be differentiated from students who remained nonsmokers using the three knowledge variables and two psychological variables for which significant changes were evident. The results of the classification analysis indicated that 73% of the students were correctly classified when the monthly measure of smoking status was used as the criterion variable and 86% of the cases were correctly classified when the weekly measure of smoking status was used as the criterion variable.

One year later these two groups of students were posttested again and the two groups compared in terms of smoking status. Although the experimental and control groups still differed with respect to all new smoking (24% vs. 32%), this difference was no longer significant. However, when more regular cigarette smoking (one or more times per week) was examined, significant differences were evident between the experimental group (11%) and the control group (25%). This study demonstrated a 58% reduction in new smoking at the initial posttest and a 56% reduction in regular smoking at the one year follow up.

The results of this study were particularly encouraging for a variety of reasons. First, the comparison of the treatment and control groups with respect to both enhanced self-reports of cigarette smoking and SCN levels indicated that the prevention program was able to significantly reduce new cigarette smoking. Second, the results of the one-year follow up indicated that the initial reductions in new experimental smoking produced at the end of the prevention program (without any additional intervention activities) resulted in significant reductions in regular smoking (using the weekly measure) at the time of the one-year follow-up. Finally, both the finding that the prevention program produced significant changes consistent with non-smoking on five of the seven hypothesized mediating variables at the time of the initial posttest, and the results of the discriminant function analysis, provide support for the construct validity of this type of prevention program.

The third study with this prevention approach (Botvin et al. 1983) was designed to provide a "real-world" test of this smoking prevention strategy when implemented by regular teachers under typical classrooms conditions. Additional objectives of this study were to test the effects of two different program scheduling formats and the extent to which booster sessions could enhance program effects. Seven suburban New York schools (N = 902) were randomly assigned to one of three conditions: (1) LST prevention program conducted once a week for 15 weeks (weekly scheduling format); (2) LST program conducted several times a week for about 5 weeks (intensive scheduling format); and (3) a control condition. Two schools were assigned to each treatment condition and three schools were assigned to the control group.

As in the previous study, saliva samples were collected using the bogus pipeline procedure in order to enhance the quality self-report data. All students were pretested and posttested by questionnaire for self-reported smoking status and on several hypothesized mediating variables, including smoking knowledge, psychosocial knowledge, assertiveness, locus of control, social anxiety, self-esteem, self-confidence, self-satisfaction, smoking assertiveness (tendency to refuse offers to smoke cigarettes), decision-making autonomy, and smoking attitudes.

Comparison of the combined experimental group and the control group revealed significant differences in the proportion of new smokers (6% vs. 13%). No significant differences between the two scheduling formats were apparent at the initial posttest. The impact of the prevention program on mediating variables was determined using analyses of covariance, with pretest scores being used as covariates. Significant treatment effects were found for smoking knowledge, psychosocial knowledge, general assertiveness, locus of control (for the intensive scheduling format only), smoking assertiveness, decision-making autonomy, and smoking attitudes (for the intensive format only). A sex by treatment interaction was found for social anxiety, self-confidence, and self-satisfaction. All of these effects were in a direction hypothesized to be consistent with nonsmoking.

At the one-year follow up, comparison of the combined experimental group indicated that the program resulted in significantly fewer new smokers using the monthly recall measure (15% vs. 22%), the weekly measure (8% vs. 15%), and the daily measure (6% vs. 11%). Results also indicated that the intensive approach was more effective for all measures (monthly, weekly, and daily) of smoking status. Finally, the results indicated that providing students with additional "booster" sessions in the eighth grade can help to maximize the effectiveness of the prevention program. The booster program consisted of eight sessions and was designed to reinforce the material covered in the seventh grade program. The best nonbooster group had an onset rate for regular smoking of 9% (compared to a 15% rate for the controls), while the booster group had an onset rate of only 2%. Despite the relatively low number of schools per condition, additional statistical analysis of the smoking data from this study indicate the presence of strong treatment effects which are independent of any unmeasured "school" effects (see Botvin et al. 1983). However, one limitation of this study is that since only once school received the additional booster sessions, the booster effects reported were confounded with school effects.

Therefore, based on the most inclusive measure of new smoking (monthly smoking), the prevention program resulted in a 50% reduction in new cigarette smoking at the end of the first year (6.5% vs. 13%). Although both scheduling formats for implementing the LST program produced essentially the same results at the end of the first year differences emerged at the one-year follow up. The intensive scheduling format resulted in

a 55% reduction in new cigarette smoking at the end of the second year (10% vs. 22%), while the less frequent (weekly) scheduling format was not significantly lower than the control group (19% vs. 22%). For the students receiving additional booster sessions, new regular smoking was reduced by 87% at the end of the second year (2% vs. 15%). Once again, treatment effects were evident for several of the hypothesized mediating variables at the one-year follow up, including smoking knowledge, psychosocial knowledge, locus of control, self-satisfaction, smoking assertiveness (for the intensive format only) and smoking attitudes (for the intensive format only).

Research is currently underway to test the impact of the LST prevention strategy on alcohol and marijuana use. Since tobacco, alcohol, and marijuana use not only appear to be promoted by the same etiologic factors but also occur at roughly the same point in the developmental sequence of substance use behavior, it was hypothesized that this type of broad-spectrum prevention strategy would also have an impact on alcohol and marijuana use. A secondary goal was to test the relative effectiveness of this type of prevention program when implemented by either older (10th and 11th grade) peer leaders or by regular classroom teachers.

The study includes 1311 7th grade students from 10 suburban New York junior high schools. The students in these schools are predominantly from white middle-class families. Two schools were randomly assigned to each of the following five conditions: (1) teacher-led LST program, (2) peer-led LST program, (3) teacher-led LST program plus booster sessions, (4) peer-led LST program plus booster sessions, and (5) control. The LST prevention program was conducted in 20 sessions over a four month period. The booster program consisted of eight sessions in the eighth grade and five sessions in the ninth grade conducted over a three to four week period each year.

Saliva samples were collected at both the pretest and posttest followed by the administration of a questionnaire containing several items on self-reported behavior, knowledge, and attitudes concerning smoking, drinking, and marijuana use. Assertiveness, locus of control, social anxiety, self-esteem, smoking assertiveness, and decision making independence were also assessed by questionnaire. Approximately four months after the pretest, all students were posttested by questionnaire and saliva samples were once again collected.

Results of the first year of this study (Botvin et al. 1984) indicate that the prevention program had a significant impact on tobacco, alcohol, and marijuana use at the posttest. The students in the peer-led condition reported drinking significantly less alcohol per occasion than either the students in the control condition or the teacher-led condition. Perhaps most dramatic was the impact of the prevention program on marijuana use. Not only were there significantly fewer students reporting marijuana use on both the monthly and the weekly recall

measures, but the magnitude of these differences was quite substantial. Comparing the proportion of students reporting marijuana use in the peer-led condition with the control condition, the prevention program reduced marijuana use by 71% (2% vs. 7%) using the monthly recall measure and by 83% (1% vs. 6%) using the weekly recall measure.

As was the case in the previous studies, significant changes were also evident with respect to several hypothesized cognitive, attitudinal, and personality mediating variables in a direction consistent with non-substance use. Comparison of the peer-led LST condition, teacher-led LST condition and the control condition for the hypothesized mediating variables using analysis of covariance revealed significant treatment effects for several of these variables. For the peer-led condition, significant treatment effects were found for smoking knowledge, drinking knowledge, marijuana knowledge, smoking attitudes, drinking attitudes, marijuana attitudes, locus of control, and smoking assertiveness. Treatment effects were found for only three of these variables in the teacher-led condition. These included significant increases in both smoking knowledge and marijuana knowledge and an unexpected increase in social anxiety.

Overall, the research conducted with the Life Skills Training approach has indicated that it is an effective substance abuse prevention strategy. Although the results of the very first (pilot) study using the Life Skills Training prevention strategy produced promising results, interpretation of these results was seriously limited due to the reliance on conventional self-reports of smoking status. However, subsequent studies with this prevention strategy have all utilized enhanced self-report data (and in one case included saliva thiocyanate levels as an additional dependent variable) and have all indicated that the LST program is capable of producing initial reductions of 50% or more in new cigarette smoking among junior high school students.

These initial reductions in relatively infrequent experimental smoking have been found to result in reductions in regular smoking of approximately the same magnitude one year later without any additional intervention activities. Still, it is evident that some erosion of the original effects of the prevention program occurs over time, suggesting the need for continued intervention. Furthermore, this type of prevention program has been found to be effective when implemented by members of the project staff, by older peer leaders, and by classroom teachers. The results of one study suggest that booster sessions may be able to facilitate the maintenance of initial program effects and may even enhance them. In the most recent study conducted, this type of prevention program has also been found to have a significant impact on both drinking and marijuana use.

Finally, the studies conducted thus far have indicated that this prevention approach can produce significant changes consistent

with non-substance use on such hypothesized mediating variables as knowledge and attitudes relating to smoking, alcohol, and marijuana use; assertiveness; locus of control; social anxiety; self-satisfaction; self-esteem; and decision-making autonomy. All of these changes have been in a direction consistent with the theory underlying this prevention model suggesting that substance abuse may be prevented through a strategy which enhances the development of generic personal and social life/coping skills as well as teaches information and skills related more directly to social influences to smoke, drink, or use drugs. However, many of these measures provide only a relatively indirect assessment of the extent to which the LST program increased life/coping skills. Furthermore, there have been differences across studies in the measures used to assess specific variables. Thus, it is difficult to draw any firm conclusions about the construct validity of this prevention model at this point.

GENERAL DISCUSSION

Four substance abuse prevention models and a total of nine reported evaluation studies have been reviewed in this paper. These models were selected for review because they represent broader-based approaches to substance abuse prevention than prototypic inoculation/pressure resistance approaches. These four prevention models all have similar theoretical roots and utilize intervention techniques derived largely from cognitive-behavior therapy. Substance abuse prevention is approached indirectly through interventions designed to enhance generic personal and social skills, although the specific application of these skills to resisting substance use pressure is also included in most cases.

Despite these important similarities, differences exist concerning the range of personal and/or social skills included in these programs. All four prevention programs include components dealing with assertiveness; three of the prevention approaches include material dealing with both decision making/problem solving and information related to substance use; two programs include components dealing with anxiety/stress reduction; and one program includes a component dealing with general social skills.

Differences also exist in characteristics of the intervention program such as target age group, program length, frequency of sessions, the primary provider, and whether or not booster sessions are included. Four of the studies reviewed in this paper were implemented with 7th graders; three were implemented with 6th graders; one was implemented with 6th, 7th, 8th, and 9th graders; and one was implemented with 8th, 9th, and 10th graders. Program length ranged from as few as seven sessions to as many as 20 sessions. Some of these intervention programs were conducted at a rate of one class session per week, while others were conducted at a rate of two or more classes per week. All of the studies conducted so far, with the exception of two (Botvin and Eng 1982; Botvin et al. 1984), used adult primary providers. In

some cases these adults were teachers and in other cases they were outside health professionals (i.e., project staff members, graduate interns, social workers). The majority of the intervention studies have not included booster sessions.

Finally, the evaluation studies conducted thus far have major differences in terms of design, objectives, populations, dependent measures, sample size, and length of follow up (see table 3). Five of the nine studies reviewed simply tested the effectiveness of the intervention program. However, the other four studies were designed so that they could provide information on issues relating to the content of the intervention program (Pentz, in press; Schinke and Gilchrist, in press), scheduling format (Botvin et al. 1983), the relative effectiveness of peers versus teachers as primary providers (Botvin et al. 1984), and the effectiveness of booster sessions (Botvin et al. 1983; Botvin et al. 1984). Some of the studies reviewed were pilot studies involving a small number of students from 1 or 2 schools, while others were larger scale studies involving 800 to 1,000 students from 7 to 10 schools.

Accomplishments

Differences such as those mentioned above make the task of drawing conclusions about these prevention models somewhat complicated. However, some things can be said concerning these approaches as a whole. First, all of the evaluation studies have produced evidence that these prevention models are capable of having an impact on one or more substance use behaviors. The most common behavioral outcome is in terms of the effect of this generic type of prevention approach on cigarette smoking. In fact, all of the evaluation studies reviewed, except one (Pentz, in press), produced a significant treatment effect on cigarette smoking. Two studies (Pentz, in press; Botvin et al. 1984) have reported an impact on alcohol use and one study (Botvin et al. 1984) has reported an impact on marijuana use.

Second, the magnitude of the reported effects appears to be relatively large. In general, these studies have demonstrated that generic skills approaches to substance abuse prevention can produce about a 50% reduction in the incidence of substance use behavior. Two studies produced reductions in smoking initiation of 75% to 80% (Botvin et al. 1980; Schinke and Gilchrist 1983), and one study (Botvin et al. 1983) reported an 87% reduction in the initiation of regular smoking for students who participated in additional booster sessions. Overall, these effects are comparable to or greater than those reported in the studies evaluating the efficacy of the inoculation/refusal skills approaches.

Third, a major distinguishing feature of the evaluation studies reviewed in this chapter is that they have attempted to measure the impact of broad-based intervention programs on presumed mediating variables. All of these intervention approaches have

produced measurable effects on a spectrum of hypothesized mediating variables. Although the impact on specific mediators has not been completely uniform across studies (see Glasgow, this volume), the observed effects have all occurred in the hypothesized direction and in a manner consistent with behavioral effects. This is clearly an important step toward understanding why these programs are effective. However, much more research is necessary in order to be able to confidently draw conclusions about how and why these programs work.

Fourth, several of these studies have produced results indicating that broader-based generic approaches may be more efficient since they can have an impact on several related behavioral domains at the same time. For example, Botvin et al. (1984) reported significant effects on smoking, alcohol use, and marijuana use. Pentz (in press) reported that her intervention program had a positive effect on students' academic performance. Schinke and his colleagues have found that an approach similar to the one used in preventing cigarette smoking was also effective as a strategy for changing variables related to the prevention of teenage pregnancy.

Fifth, these studies provide information concerning the conditions under which this type of prevention approach can be effective. These studies indicate that generic personal and social skills substance abuse prevention programs can be effective whether the primary providers are project staff, social workers, graduate interns, peer leaders or classroom teachers. These approaches have also been found to be effective with rural, suburban, and urban students. One study suggests that a more intensive programming format—involving frequent sessions over a relatively short time span—may be more effective than spacing sessions out over a somewhat longer time span, and that booster sessions may help maintain and even enhance program effects.

Methodological Issues and Potential Limitations

Despite the emphasis on evaluation which has characterized both the inoculation/refusal skills substance abuse prevention approaches and the broader personal and social skills training approaches discussed in this chapter, researchers working in this area recognize the need to continue to strengthen the methodological rigor of their studies. Interpretation of the results of the studies reviewed in this chapter is limited by some of the same methodological problems found in the evaluation studies testing the effectiveness of the inoculation/refusal skills approaches reviewed by Flay (this volume). However, as a group, the studies conducted with more generic skills training approaches have a number of important strengths.

One of the most important methodological issues relates to the quality of self-report data and the collection of biological samples (e.g., SCN). Concerns about the validity of self-report data have been raised previously (e.g., Evans et al. 1977). The

extent to which the quality of outcome data are suspect obviously limits the interpretability of individual evaluation studies. All the studies reviewed in this chapter, except for the pilot study conducted by Botvin and his colleagues (Botvin et al. 1980) and the study conducted by Pentz (in press), have collected biological samples and have utilized enhanced self-reports as the primary dependent measure. One study (Botvin and Eng 1982) included the analysis of SCN levels in addition to the analysis of self-report data, and found evidence of program effects on both measures. Thus, confidence can be reasonably high that the data utilized in these studies are valid.

Another important methodological issue concerns both the method of assignment and the unit of analysis. All of these studies have used random assignment. However, the unit of assignment has varied from schools to classes to individuals. The majority of studies have used the school or classroom as the unit of assignment while conducting analysis on the individual level, confounding potential school or classroom differences with treatment effects. To some extent, this type of threat to internal validity has been mitigated in several of these studies (Botvin et al. 1983; Botvin et al. 1984; Pentz, in press; Schinke and Gilchrist, in press) by the assignment of two or more units to each condition. Although researchers working in the area of substance abuse prevention are cognizant of this problem, efforts to solve it have generally been hampered by the need for a large number of schools or classrooms.

Two other problems which can compromise internal validity and hinder the interpretability of the results of these studies are pretest non-equivalence of conditions and differential attrition. These two issues have generally not been directly addressed in the research reports of these studies. However, examination of the data presented in the reports of these studies indicates that pretest non-equivalence of experimental conditions has not generally been a problem. The issue of attrition is somewhat more complicated. While few of the reports of these studies provide data on attrition, it is obvious that attrition has occurred in virtually all of these studies. However, more serious than the actual amount of attrition is the extent to which there is differential attrition among experimental conditions. Unreported attrition analysis recently conducted on the Botvin et al. (1983) data indicated that although attrition was more likely to occur among smokers, there was no evidence of differential attrition across conditions. The results of similar analyses for the other studies reviewed in this chapter are not available, leaving open the question of whether or not differential attrition compromised the internal validity of any of these studies.

Notwithstanding the presence of some methodological shortcomings, it is important to recognize that the research conducted with generic personal and social skills training approaches (as well as the research conducted with the inoculation/refusal skills

approaches) has become progressively more rigorous. Pilot studies have generally involved a small number of schools and been less methodologically sophisticated. Encouraged by initial successes and cognizant of the shortcomings of earlier studies, more recent studies have been larger and better designed. It is also important to recognize that, despite the increased methodological rigor of the most recent studies, the results obtained are quite similar to the results obtained in the earlier studies. This consistency of results across studies (particularly with successive studies testing the same intervention model) provides considerable cause for optimism.

Summary

A total of nine evaluation studies have been conducted with four prevention models which focus on teaching generic personal and social skills. All of these approaches have produced demonstrable effects for one or more substance use behaviors, and the magnitude of these effects generally appears to be large. Although methodological problems may exist in some of these studies, the magnitude of the effects obtained and their consistency across studies provide strong support for the efficacy of these approaches.

Finally, a major strength of the research conducted in this area is that all of these studies have assessed the impact of the prevention programs on presumed mediating variables, and have been able to demonstrate effects on a number of these variables consistent with the behavioral results. Thus, while these studies collectively provide evidence for the effectiveness of generic personal and social skills training substance abuse prevention models, only preliminary data are available concerning how and why these programs work.

RECOMMENDATIONS FOR FUTURE RESEARCH

The results of research with these kind of generic personal and social skills training approaches to substance abuse prevention are extremely encouraging. However, a number of important issues need to be examined further.

First, most of the research conducted thus far with this type of prevention strategy has focused on cigarette smoking. Although some studies more recently have examined the extent to which the broader personal and social skills training approaches impact on other substances, more research should be conducted to increase our understanding of their effectiveness as alcohol and drug abuse prevention strategies.

Second, future studies should continue to give high priority to process analysis, assessing the impact of these prevention programs on the specific skills, knowledge, attitudes, etc. hypothesized to play a role in substance abuse prevention. For example, if the prevention program includes a component designed

to teach students techniques for coping with anxiety, an effort should be made to determine the extent to which these skills are both learned and utilized. These efforts will provide additional information concerning how and why these prevention programs work, and will also enable investigators to refine the theoretical models guiding their research.

Third, virtually all of the substance abuse prevention research has been conducted with predominately white, middle-class populations. Future research needs to be conducted to determine the extent to which these programs are also applicable to those low SES populations likely to be at high risk for becoming substance abusers.

Fourth, future research should attempt to identify the "active ingredients" of this type of prevention strategy in order to determine whether or not the kinds of general coping skills taught actually contribute significantly to program effectiveness. One approach to this issue would be to conduct studies which provide for the testing of the various components of these programs both alone and in combination.

Fifth, future research should attempt to identify the factors that can either positively or negatively affect program outcome. This would involve examining issues such as the relative efficacy of different types of primary providers, different methods of training primary providers, and different program formats. In addition, future studies should include measures of environmental variables, such as community characteristics and school atmosphere, and these variables should be related to program outcome effects.

Sixth, these prevention strategies should be tested against other substance abuse prevention models in order to determine their relative efficacy and cost-effectiveness. The most obvious example of this type of investigation would be one comparing the type of generic personal and social skills prevention strategy discussed in this chapter with the more focal type of prevention strategy discussed by Flay in this volume.

Seventh, it will be necessary to learn more about how to insure successful implementation of these kind of programs in the absence of highly motivated researchers. It will be important to determine the extent to which these kind of prevention programs can be effectively implemented by school personnel or other individuals in the community who would be likely to conduct these programs on an ongoing basis. Research studies will also need to determine the kind of training and ongoing support necessary to foster the successful implementation of these programs, as well as the nature and type of curriculum materials.

Finally, it will be necessary to conduct large-scale "clinical" trials to determine the effectiveness of these prevention strategies when implemented with a broad range of students.

Table 1
Intervention Components

Focus of Intervention Components	Pentz	Schinke	Wills	Botvin
Assertiveness	X	X	X	X
General Social Skills				X
Anxiety/Stress Reduction			X	X
Decision Making/Problem Solving		X	X	X
Information Related to Substance Use		X	X	X

Table 2
Intervention Characteristics

Study	Grade	# of Sessions	Frequency of Sessions	Primary Provider	Boosters
Botvin et al. 1980	8,9,10	10	weekly	staff	no
Botvin and Eng 1982	7	12	weekly	older peers	no
Botvin et al. 1983	7	15	weekly, 3 per week	teachers	yes
Botvin et al. 1984	7	20	1-2 per week	peer vs. teacher	yes
Schinke and Blythe 1981	6	8	2 per week	graduate interns	no
Schinke and Gilchrist 1983	6	8	2 per week		no
Schinke and Gilchrist, in press	6	8	2 per week	social worker	no
Pentz, in press	6,7,8,9	7	weekly	teacher & staff	no
Wills, in press	7	8	4 per week	staff	yes

Table 3

Methodological Characteristics of Studies

Study	Experimental Tests	# of Students	# of Schools	Type/Unit of Assignment	Unit of Analysis	Dependent Measures ¹	Biological Sample	Assessment of Mediating Variables	Student IDs	Pretest Differences	Longest Follow-Up
Botvin et al. 1980	P vs. C	281	2	random/school	individuals	smoking	no	yes	yes	no	3 months
Botvin and Eng 1982	P vs. C	426	2	random/school	individuals	smoking, SCN	saliva	yes	yes	no	1 year
Botvin et al. 1983	P (weekly) vs. P (integrated) vs. C ²	902	7	random/school	individuals	smoking	saliva	yes	yes	no	1 1/2 years
Botvin et al. 1984	P (teacher-led) vs. P (peer-led) vs. C ¹	1,311	10	random/school	individuals	smoking, alcohol, marijuana	saliva	yes	yes	yes ⁷	-- ³
Schinke and Blythe 1981	P vs. C	28	1	random/student	individuals	smoking	saliva ⁶	yes	yes	no	6 months
Schinke and Glichrat 1983	P vs. C	56	2	random/student	individuals	smoking	saliva ⁶	yes	yes	no	6 months

Table 3
(continued)

Study	Experimental Tests	# of Students	# of Schools	Type/Unit of Assignment	Unit of Analysis	Dependent Measures ¹	Biological Sample	Assessment of Mediating Variables	Student IDs	Pretest Differences	Longest Follow-up
Schinke and Gilchrist, in press	P (info + skills) vs. P (info + attitude) vs. C	234	2	random/classroom	individuals	smoking	saliva	yes	yes	no	1 year
Pentz, in press	P (full) vs P (partial) vs. C ⁵	1,193	8	random/classes	individuals	smoking, alcohol	no	yes	yes	no	6 months
Wills, in press	P vs. C	800	3	random/school	individuals	smoking, alcohol	saliva	yes	yes	no	10 months

Notes:

- (1) self-report measures were used unless indicated otherwise
- (2) the intensive scheduling format condition was divided into a booster/non-booster condition during the second year of the study with one school in each condition to provide a pilot test of the effectiveness of booster sessions
- (3) 1 year follow-up data are currently being analyzed; at the conclusion of the study 2 and 3 year follow-up data will be available
- (4) each of the two treatment conditions have been randomly assigned to booster/non-booster conditions
- (5) the relative effectiveness of the two treatment conditions were also assessed in terms of their impact on rural and suburban populations
- (6) saliva samples were collected only at the 6 month follow-up
- (7) data were analyzed using adjusted proportions, with pretest responses being used as covariates

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Social and Personal Skills Training Programs for Smoking Prevention: Critique and Directions for Future Research

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The purpose of this paper is to react to the paper on social skills training by Botvin and Willis (this volume), commenting on both accomplishments and shortcomings of this line of research. Before this can be done, it is first necessary to delineate the critical or unique aspects of this approach to smoking prevention. We will then discuss the advantages and disadvantages of a broad-spectrum, general competence approach compared to more narrowly focused prevention programs. Rather than discussing social skills training--which is employed by virtually every recent smoking prevention program--we will restrict our focus to the broad based social skills training approaches discussed by Botvin and Willis (this volume). What is unique about these programs is not their emphasis on social skills among adolescents, but their methods for enhancing social competence. Although the two major research groups in this field (based at Cornell and the University of Washington) use the term "Life Skills Training" to describe their work, we prefer the broader and more generic term social and personal skills training.

After discussing the potential pros and cons of a broad-spectrum social and personal skills training approach, we will turn to a methodological/empirical critique of the current literature on this topic. Two basic questions will be examined: Do such programs work?; How do such programs work? Finally, we will summarize the current status of social and personal skills training approaches and provide a number of recommendations for future research in this area.

Distinguishing Features of the Social and Personal Skills Approach

From a theoretical perspective, one of the major assumptions behind the social and personal skills training (SPST) approach is that of a syndrome of adolescent problem behaviors including smoking, the use of other drugs, and other deviant behaviors such as truancy and early sexual behavior (Botvin and Willis, this volume; Jessor 1982). Smoking is viewed as one of a class of functionally equivalent behaviors through which adolescents express themselves, cope with anxiety, and deal with the

transition from childhood to adulthood. Furthermore, it is postulated that the same cluster of relatively stable underlying cognitive and personality variables (e.g., low self-esteem, high anxiety levels, poor decision making skills) predisposes adolescents toward the use of alcohol and other drugs and other problem behaviors (Botvin et al. in press; Jessor 1982). In fairness to the proponents of this model, it should be stressed that this is not a naive personality trait theory approach. Advocates of the SPST approach do consider and address social and environmental factors; they operate from a person-environment interactionist perspective. Still, what distinguishes the SPST approach from other current prevention approaches is a special emphasis on underlying individual differences.

It follows from this theoretical perspective that two distinguishing features of SPST intervention programs are (1) the utilization of therapeutic strategies to modify this underlying core of intrapersonal cognitive and personality factors and (2) a focus on general life skills thought to determine the use of various substances (e.g., alcohol, marijuana) and the manifestation of other behaviors such as precocious sexual activity, delinquency and poor performance in school. Figure 1 summarizes the theoretical and treatment components involved in the SPST approach. The left column of this figure lists the major SPST intervention components employed, the center column lists presumed mediating variables and the right column lists behaviors making up the syndrome of problem behaviors.

Potential Advantages and Disadvantages to SPST

The complexity and broad-spectrum nature of the SPST approach is the source of both its advantages and disadvantages. In terms of conceptual advantages, the inclusion of personality factors in the SPST model and the resulting Person x Situation perspective may increase the chances of modifying the most important factor or factors influencing smoking for a given individual. If we accept the premise that different individuals smoke for different reasons (Glasgow and Bernstein 1981; Pechacek 1979), it seems logical that an approach which addresses a variety of factors will be more likely to address critical factors for a larger percentage of adolescents than will a more circumscribed approach.

A second potential advantage is that SPST may be particularly effective with adolescents who are most predisposed toward substance abuse. More narrowly focused social inoculation interventions may be ineffective in providing adolescents with sufficient skills for resisting social pressures to smoke if they are not at least moderately socially competent to begin with. With its more intensive focus on general skills and competencies, SPST may provide the necessary preconditions for training in specific refusal skills to be effective.

The above arguments suggest that the SPST approach might produce greater reductions in smoking rates than social inoculation

<u>SPST Components</u>	<u>Mediating Cognitive and Personality Variables</u>	<u>Syndrome of Adolescent Problem Behaviors</u>
Goal Setting and Self-Management Strategies	Self-Esteem	Cigarette Smoking
Homework Assignments	Locus of Control	
General Assertiveness and Communication Skills Training	Assertiveness	Marijuana Use
Anxiety Reduction Procedures	Social Anxiety	Alcohol Consumption
Decision Making/ Problem Solving	Influencibility	
Self-Instruction or Self-Statement Procedures	Need for Approval	Precocious Sexual Activity
	Impulsivity	Delinquency
----- Smoking Specific Social Skills Training		

FIGURE 1

Intervention Components, Hypothesized Mediating Variables,
and Adolescent Problem Behaviors Discussed in SPST Approaches

approaches. A more compelling advantage of SPST programs is that they may lead to superior generalization and maintenance of treatment effects. If one is successful in altering stable characteristics such as self-esteem, sense of personal control, and decision making ability, such changes should produce changes in a number of aspects of one's life in addition to smoking behavior. Over the course of the adolescent years, one is exposed to a variety of problem situations, challenges and difficult decisions. Changed ways of viewing the world would be expected to result in improvements in areas such as school performance and interactions with peers and adults. One particularly interesting generalization issue is whether subjects undergoing SPST are more likely to change their peer group than are subjects in other prevention programs. If subjects undergoing SPST experience significant cognitive and personality changes, it seems that they would also be likely to change the friends that they associate with most closely.

A final potential advantage is that if the same factors underlie the use of various substances, then it should only be necessary to develop a single substance abuse prevention program, instead of a different program for each substance. It is likely that school administrators will be much more interested in offering one substance abuse program with broad effects than in having to support and find time for cigarette smoking prevention programs in addition to drug and alcohol abuse prevention programs.

Unfortunately, the complexity of the SPST approach could lead to problems that outweigh the potential advantages discussed above. It may be, for example, that adolescents will feel overwhelmed with the range of issues being discussed or the number of life changes being advocated. A related point is that if one is attempting to teach several different life skills, there may not be enough time to do a thorough job in any one area and the end result may be a superficial introduction to many different issues rather than indepth work in any particular area. The adult smoking cessation literature is replete with studies in which simpler treatment programs resulted in better outcomes than more complex programs (Lando 1981; Danaher 1977). Our own research group has found similar results and has identified at least one reason for this somewhat counter-intuitive finding. We found that individuals receiving complex multicomponent smoking cessation programs were less likely to adhere to treatment recommendations (Glasgow et al. 1981). Of course this finding should not be surprising given the well-established inverse relationship between treatment complexity and adherence found in the literature on compliance to medical regimens (Haynes 1979). The implication of these findings for SPST is that adolescents receiving a complex intervention such as SPST may not follow through on many of the homework assignments or lifestyle changes recommended.

A conceptual problem concerning SPST is that at present we may not know enough about underlying personality determinants of smoking or about the best ways of altering such factors. When discussing individual differences in their recent review of psychosocial influences on the decision to smoke, Evans and Raines (1982) commented that the identification of personality traits underlying smoking was "...an elusive goal" and on "the patchwork quality of existing knowledge" in this area. They echoed Williams' (1971) earlier conclusion that "both the empirical results of previous studies and discussions of the state-of-the-art of research into personality correlates suggest that personality will not provide the most fruitful approach to understanding why children do or do not take up cigarette smoking" (p. 112). Even individuals identified with the SPST approach have questioned the adequacy of this knowledge base. In their book on Life Skills Counseling, Schinke and Gilchrist (1984) state:

Numerous researchers have tried to distill a consistent psychosocial profile for identifying adolescents most likely to become substance users.

No consensus, however, has emerged. There appears to be no simple pattern of factors that explains or predicts adolescent drug use (p. 71).

Of course, there are others (e.g., Jessor 1982) who have reached different conclusions on this issue. Without going into a lengthy discussion of the prediction literature, we'd just like to raise the question of whether or not the type of personality and cognitive variables stressed by the SPST approach have been consistently and strongly related to adolescent smoking behavior.

Even if there was consensus about the important intrapersonal variables underlying smoking, it is not at all clear that we know how to effectively modify such factors. As Jessor (1982) notes:

In explanatory research, it is possible, for example, to demonstrate that adolescent involvement in marijuana use varies with exposure to peer group norms that support such drug use. Although establishment of that connection certainly helps us to understand some of the variation in marijuana use, it does not help us to know how to change those peer group norms (p. 448).

There is evidence that attempts to enhance specific social skills among adolescents have been relatively successful (Pentz and Tolan 1984). However, it is a far more ambitious task to attempt to modify factors such as self-esteem and feelings of personal autonomy. Many individuals have spent years in therapy attempting to accomplish such goals. In addition to issues concerning the psychometric adequacy of measures of such concepts, there is not much research which compares different approaches for modifying such factors.

A final disadvantage of the SPST approach is that it is costly and time consuming. Intervention contact time in reported studies has varied from 7-20 hour long sessions, which is about twice the length of many social inoculation prevention programs. In terms of dissemination issues, it may be more difficult, even with a detailed and well developed training manual, to train group leaders in this approach than it is for more focal prevention programs. In support of this point, the paper by Botvin et al. (in press) speculates that poor implementation may have been responsible for the lack of effect of their teacher led intervention. For additional training and class time to be justified, one would like to see demonstrations of the superiority of the SPST approach.

Before moving on to a discussion of the empirical status of the SPST approach, it should be noted that developers of SPST programs have attempted to guard against some of the disadvantages discussed above by providing information, role playing and feedback specific to smoking related situations. Thus, what we are often faced with in interpreting SPST studies is not a "pure"

general life skills training, but SPST supplemented by more focal smoking specific exercises and information. It is plausible that any effects of this program can be attributed to these specific techniques and not to training in general social and personal skills.

Methodological/Empirical Issues in Evaluating SPST

Table 1 summarizes the questions we will examine concerning the empirical status of the SPST approach. We will not address issues such as biochemical validation of smoking status, methods of assigning subjects to conditions, units of analysis in smoking prevention programs, or effects of attrition on results. While these issues are certainly relevant to the SPST area, they are already dealt with at length in Biglan and Ary's chapter (this volume). We will ask two sets of questions, one concerning the effectiveness of SPST and the second concerning how and why these programs may work. By way of overview, there are far more answers to the first set of questions than to the second.

The first question asks whether SPST produces results superior to no treatment or standard school health education control conditions. The answer to this question is yes. Botvin and colleagues have conducted four large scale studies, all of which found significantly lower smoking rates in SPST conditions. Both Schinke and colleagues (Schinke and Blythe 1982; Schinke and Gilchrist 1983) and Pentz (1983) have reported similar results. While each of these studies can be criticized for various methodological problems, the consistency of effects across studies is impressive (see Botvin and Wills this volume). Unfortunately the answer to the second question, concerning the relative efficacy of SPST compared to other smoking prevention programs, is that we simply don't know. No study has investigated this question.

The third question, concerning the magnitude of effects produced by SPST, can be viewed in several ways. One way to evaluate the meaningfulness of SPST program effects is to compute the percent reduction in smoking initiation rates for experimental conditions relative to control conditions, as Botvin has done. This approach produces impressive results: from 50-87% reductions in smoking rates. An alternative method is to examine the differences in the percentages of adolescents who smoke in experimental versus control conditions. This type of analysis, calculated on the same set of studies, reveals reductions ranging from 3 to 14%--a less impressive figure. We feel that the second procedure is more informative. There are important differences between reducing smoking initiation rates from 20% to 10% versus from 2% to 1%, but this distinction is lost using the first procedure as both sets of figures would indicate a 50% reduction. A third way of evaluating the magnitude of effects produced by SPST is to compare experimental-control condition differences reported in SPST studies to those found in studies of other prevention approaches. Comparison with results typically reported by the Houston,

TABLE 1

Methodological/Empirical Criteria for Evaluating
Social and Personal Skills Training

I. How Well Do SPST Programs Work?

- A. Does SPST reduce smoking relative to:
 - 1. No treatment or standard health education classes?
 - 2. Alternative psychosocial smoking prevention programs?
- B. How large are the effects produced by SPST?
- C. Does SPST result in better generalization of effects to:
 - 1. Use of other substances?
 - 2. Other areas of life (e.g., school performance, difficulties with the law)?
- D. Is SPST cost-effective relative to other approaches?

II. How Do SPST Programs Work?

- A. Does SPST produce changes in variables hypothesized to mediate outcome and are changes in these variables related to smoking status?
 - B. What are the critical components of the SPST package?
 - C. What is known about conditions or factors moderating the effects of SPST?
-

Minnesota, Oregon and University of Southern California groups reveals that the SPST approach generally produces results comparable to and sometimes larger than these other programs. Once a sufficient number of studies have been conducted, this type of comparison could be improved by formal computation of effect sizes for use in meta-analysis studies (e.g., Cook, this volume; Strube and Hartman 1983).

Before moving on to the next question, the effects reported by

Schinke and colleagues (Schinke and Blythe 1982; Schinke and Gilchrist 1983) are worthy of special comment. They have obtained significant differences between conditions with sample sizes of 14-28 subjects per condition; in this area, such results are truly amazing. Although their reports do not specify details regarding participants, the subjects in these studies report extremely high rates of cigarette smoking. For example, at a one-year follow-up, Schinke and Gilchrist (1983) reported that 8% of the 7th grade subjects in their experimental condition reported smoking, compared to 37 1/2% of the controls! Effects of this magnitude are certainly worthy of further investigation. Further, these findings suggest that the program had a sizable effect with a group of adolescents strongly predisposed toward cigarette smoking.

Question I.C. in table 1 concerns generalization of treatment effects and relates to one of the potential advantages of SPST discussed earlier. SPST investigators have recently begun to investigate whether or not treatment effects generalize to the use of other substances. It does appear that the SPST approach produces reduced rates of alcohol and marijuana usage, as well as lower rates of cigarette smoking (Botvin et al. in press; Pentz 1983). However, it is not clear whether such results should truly be considered generalization effects since experimental subjects did receive specific information and social skills training pertaining to those other substances. There has been very little research on the issue of generalization of SPST effects to adolescent "problem behaviors" besides substance use. Pentz (1983) has reported potentially important findings regarding improvements in students' grade point averages following intervention, but treatment carry-over into other realms such as early sexual behavior or delinquency have apparently not yet been investigated.

Due to the lack of comparative studies of SPST versus other approaches, there is no direct information available on the relative cost-effectiveness of SPST programs. It would seem that a central question for future research, as well as for potential users, should be the trade off between the additional time and expense to offer the somewhat more lengthy and comprehensive SPST programs versus the potentially greater treatment generalization associated with this approach.

How and When Does SPST Work?

The second set of questions in table 1 will be discussed in somewhat greater detail because the answers to these questions have great significance for our understanding of the SPST approach. The first question, concerning hypothesized mediating variables, can be broken down into two separate questions. The first concerns whether SPST treatments produce changes in the variables hypothesized to mediate treatment outcome (see figure 1). All of the investigators whose work is discussed by Botvin and Wills (this volume) have collected data on potential

mediators. Their conscientiousness in this regard stands in contrast to the myriad of prevention studies which provide no information on why an intervention may--or may not--produce its intended effects. In particular, Botvin and colleagues have collected a number of similar process measures in several different studies.

Table 2 summarizes findings concerning comparisons of SPST to control conditions on potential mediating variables in four different studies conducted by Botvin's group (Botvin et al. 1980, 1983, in press; Botvin and Eng 1982). It is important to remember that all four of these studies found SPST reduces smoking rates relative to controls. Table 2, which illustrates the presence or absence of significant improvement relative to controls on the various measures, can best be summarized by saying differences were observed on only some of the measures only some of the time. As can be seen, there is no consistency across the four studies for the first five variables listed. Some of this variability could possibly be explained by population differences, but the last three studies were all conducted with predominantly suburban white middle class 7th grade subjects. Indeed, the only consistent findings to emerge are for locus of control and for smoking-related knowledge. Significant improvements were never found on locus of control, which is one of the more central mediating variables discussed by SPST proponents. On the other hand, smoking-related knowledge was consistently enhanced more in SPST subjects than controls. This pattern of results certainly raises questions about how SPST achieves its effects. Presumably it is not only through knowledge changes since scores of smoking prevention studies have demonstrated that increased knowledge does not result in behavior change (see reviews by Evans and Raines 1982; Thompson 1978).

To understand how SPST works, it is not enough to simply ask if changes are observed on various mediating variables. One must also ask if changes in these variables are related to changes in smoking status. It should be emphasized that even when change is observed on a mediating variable as well as on an outcome measure, it does not imply that this variable caused the change in the outcome measure. The interested reader is referred to Cook and Campbell (1979), Judd and Kenny (1981) and McCaul and Glasgow (in press) for more detailed discussions of these issues. The good news is that the data to answer these questions have already been collected. What remains is for the appropriate analyses to be conducted.

The next question in table 1 asks about the critical components of the SPST package. We know very little about the relationship of specific components of the SPST package, depicted on the left side of figure 1, to either the supposed mediating variables in the center column or to the outcome variables on the right hand side of the figure. One way of answering this question is through constructive or parametric research designs which systematically vary the presence or level of intervention procedures felt to be

TABLE 2
Significant Improvement on Hypothesized Mediating
Variables Relative to Controls in SPST Studies

Mediating Variables	Studies			
	Botvin et al. (1980) ¹	Botvin & Eng (1982)	Botvin et al. (1983)	Botvin et al. (in press)
Social Anxiety	Interaction with Sex	Yes	Interaction with Sex	No
Self-Esteem	(Self-image) No	No	No	Interaction with Sex
Influencibility	No	Yes	Yes	No
Assertiveness	NR ²	NR	Yes	No
Smoking Attitudes	NR	NR	No ³	Yes
Locus of Control	No	No	No	No
Smoking Knowledge	Yes	Yes	Yes	Yes

¹ This study reported different patterns of results across different grade levels. Results presented here are collapsed across grade levels.

² NR = Effects on this variable not reported

³ There were no differences between groups on attitudes at posttest, but there were significant differences between some (but not all) of the experimental conditions and the control condition at one year follow-up.

responsible for producing certain effects. As Cook (this volume) points out, dismantling research designs which remove hypothesized critical components are particularly appropriate for this purpose. Unfortunately, such studies have not yet been conducted and all SPST studies to date have confounded components designed to enhance general personal competence with specific smoking related information and cigarette refusal skills.

The final question in table 1 concerns whether setting factors and subject variables moderate the effects of SPST. A number of studies have answered this question affirmatively. Botvin et al. (1980; 1983) reported significant Sex by Treatment interactions; Botvin et al. (1980) and Pentz (1983) both found SPST to be differentially effective at different grade levels; and Pentz (1983) found that her intervention produced the largest effects for adolescents who were initially classified as aggressive (as opposed to passive or moderately assertive). Although setting/subject differences seem to be obtained with some consistency, there has been as yet little attempt to explain such findings. Attempts to put forth some explanations, even if they are posthoc, may eventually enhance our understanding of why and how SPST operates.

Summary and Suggestions for Future Research

The old adage that frequently accompanies review articles--that "more research is needed"--is applicable to the SPST area as well. However, we do not need more of the same. There is no rationale that justifies additional "treatment versus no treatment" studies that fail to address the issue of why SPST works. The research strategies most likely to advance the field are investigations that include experimental manipulations of major components of the SPST model and appropriate measures of how these programs achieve their effects.

In some ways, Botvin's work can serve as a model for the type of developmental work that is needed. After developing a treatment package, he and his colleagues have proceeded to test the effects of modifying this basic program by varying the scheduling of intervention meetings (Botvin et al. 1983), adding booster sessions to enhance maintenance (Botvin et al. 1983; in press), and implementing the program with peer leaders versus classroom teachers (Botvin et al. in press). They have even dared, in the last couple of instances, to put to empirical test some of the "myths, untested and erroneous assumptions" (Johnson 1982) that a large number of investigators still accept uncritically. Future SPST studies should be designed to evaluate the truth or falsity of the potential advantages and disadvantages of SPST discussed earlier.

To summarize the current status of the SPST approach in a few sentences, the results uniformly suggest that an effective smoking prevention approach has been developed. However, much more work remains to be done and, in particular, we know little about how or

why SPST seems to work. Such knowledge is important for testing the adequacy of the conceptual model on which SPST is based, to provide guidelines for determining which SPST components need to be retained and which can be discarded, and for suggesting ways in which SPST interventions can be made more effective. Of course, much the same can be said of social inoculation prevention approaches. We will now turn to more specific recommendations for future research, which are summarized in table 3. Botvin and Wills (this volume) have made this task easier for us by concluding their paper with recommendations for future research. We have listed their points in the upper part of table 3, and we will use these suggestions as a springboard for recommendations of our own.

They begin by calling for measurement of the impact of SPST on the skills and personality variables being targeted. While this is certainly needed, much more attention needs to be given to the measurement of these intervening variables. With the exception of role play assessments and observational measures of smoking refusal skills developed by Schinke and Gilchrist (1983) and Pentz (1983), the measures utilized to date have consisted of questionnaires. There are numerous questions regarding the psychometric adequacy of such measures. Two particular issues which need to be addressed in future research are the multimethod assessment of these hypothesized mediating variables and their discriminant validity. It is not clear if the variables listed in the center of figure 1 are distinct factors or different ways of measuring the same construct. Careful measurement of these mediating processes is as important for the understanding and refinement of SPST programs as is careful measurement of smoking behavior.

We have already discussed the importance of carrying this issue of underlying variables further by attempting to identify which components of the SPST package lead to changes in particular mediating variables. Study of the impact of specific program components on particular mediating variables can be efficiently accomplished in small scale, short-term analog studies (McCaul and Glasgow in press). The related question of which of these variables are in turn related to change in each of the outcome variables on the right side of figure 1 will probably require larger scale long-term trials due to the low base rates of initiation to substance use over short time intervals.

Botvin and Wills' second recommendation is that we study lower SES and non-school populations who are at risk for becoming substance abusers. We are supportive of this recommendation, but such research could be enhanced by prior consideration of what it is about different populations that should make SPST either more or less effective. Without such thinking, one could envision proposals to skip from one lower SES and non-school population to another, to propose tests in urban vs. rural areas, to try "middle" schools vs. "junior highs" and so on, ad infinitum. A related issue is that there have not been sufficient analyses in

TABLE 3

Recommendations for Future Research on SPST

Botvin and Wills' recommendations:

1. Measure the impact on skills being targeted
2. Study minority populations at high risk
3. Identify active ingredients of SPST
4. Identify factors affecting program effectiveness
5. Test SPST against competing prevention programs
6. Study ways to insure successful implementation

Other issues in need of study:

1. Additional development and validation of measures of mediating variables
 2. Investigations of relationships of mediating variables to SPST intervention components and to outcome variables
 3. Identification of which "transitions" are affected by SPST (nonsmoker to experimental smoker, etc.)
 4. Investigation of problem specific social skills training vs. general SPST
 5. Evaluation of the contribution of homework assignments to SPST and the issue of adherence to such assignments
 6. The necessity of providing anxiety management training
-

existing studies of which subset of the adolescent population most benefits from SPST. In particular, we need to know if SPST primarily reduces initial experimentation with cigarettes, decreases the number of experimental smokers becoming more regular users, and/or serves as a buffer against regular users becoming daily, habitual smokers. Again, these data are presently available, but such analyses have generally not been reported. In

fact, in their earlier studies, Botvin's group excluded anyone who had smoked at pretest (Botvin et al. 1980; Botvin and Eng 1982). It can be argued that these excluded subjects are the very students most in need of a prevention program (Biglan and Ary, this volume).

The third recommendation about identifying active ingredients of SPST is certainly indicated. However, the SPST package is so complex that more specific recommendations are in order. We have listed three issues which we feel should receive high priority for future research (see points 4-6 at the bottom of table 3). The central issue is whether or not general training in social and personal skills adds anything to the specific training in cigarette refusal skills provided by almost all present day smoking prevention programs. The study of problem-specific vs. broad-spectrum coping skills training is probably the issue most in need of study at the present time. A second under-researched issue is the role of homework assignments in SPST training. By systematic manipulation of homework assignments as well as careful measurement of adherence to such assignments, much could be learned about SPST. It seems fitting that the issue of SPST homework be investigated, since the proponents of this approach are essentially advocating adding a course in life skills and personal competence to the junior high school curriculum. Finally, an issue which might permit trimming of some of the excess fat from the complex SPST curriculum is the necessity for the inclusion of relaxation training and anxiety management procedures. At least as compelling an argument can be made for the importance of such components in adult smoking cessation programs since a majority of adult smokers report using cigarettes to reduce tension and stress. Yet, controlled investigations of the contribution of relaxation training and other anxiety management strategies to smoking cessation programs have produced negative results (see reviews by Glasgow and Bernstein 1981; Pechacek 1979).

The fourth point in the top portion of table 3 states that we should attempt to identify factors that negatively or positively affect program effectiveness. Research to date suggests that this is an important issue and that we should be cautious when making general statements about the effects of SPST. Interactions have already been identified between treatment and the following factors: grade level, gender, and urban/rural setting. Some of the developmental factors addressed by Glynn and Leventhal (this volume) should help to explain these differential results.

Botvin and Wills' fifth point is that SPST should be tested against competing substance abuse prevention programs. We disagree. It makes little sense to just compare one group's program to another for the sake of trying to conclude that one is better than or equally effective as another. The overwhelming odds are that the results of such a "horse race" comparing, say, the Cornell program to the Minnesota program, the USC program or the Oregon program would end up the same way as have the great

majority of comparative psychotherapy outcome studies: all treatments would be better than no treatment and not significantly different from each other. Also, given the relatively similar results reported across these different programs, the sample size that would be required to conduct a sufficiently powerful study would be enormous. A much more informative strategy would be to compare conceptually distinct interventions with the measurement of mediating variables predicted to be differentially affected by the different treatments.

Botvin and Wills' final point concerns implementation issues. They suggest that such research should come after the above questions have been answered. While it could certainly be argued that there is a great deal of more basic research that needs to be done before focusing attention on dissemination, implementation research could be conducted concurrently with more basic studies of treatment process. Both types of investigations can potentially reinforce each other. One implementation factor potentially worthy of investigation might be the use of computer-assisted video instruction to deliver portions of SPST programs.

In conclusion, to integrate the above myriad of recommendations, we will describe an example of the type of study which needs to be conducted. Such a study would compare training in specific social skills relevant to cigarette refusal to either general SPST training not specifically pertaining to cigarettes and/or to the combination of those two approaches. It would include carefully developed procedures to assess hypothesized critical intervening variables and it would be conducted with a sufficiently large and heterogeneous sample such that analyses could be conducted on subject and setting factors potentially affecting outcome. It would contain measures of "problem behaviors" such as the use of other substances in addition to cigarettes, assessment of changes in peer group relationships, and indices of adherence to homework assignments. Finally, the study would investigate the utility of individually tailoring components of SPST based upon subjects' scores on the mediating variables discussed above. Such tailoring could be done either initially or in a maintenance "booster" intervention.

FOOTNOTES

¹Although Schinke and Pentz have conducted far fewer studies than Botvin's group, it should be noted that they have somewhat more consistently found significant differences favoring experimental conditions on measures of problem-solving and videotaped role plays of cigarette refusal.

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What We Know About the Social Influences Approach to Smoking Prevention: Review and Recommendations

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BACKGROUND

Peer and family influences have been shown more consistently than any other factors to be primary causes of the onset of smoking among adolescents (Flay et al. 1983a). Media influences are also thought to be important, although this has not been established empirically. The social influences approach to smoking prevention focuses on one or more aspects of (a) teaching students about the social influences to smoke, (b) providing them with behavioral skills with which to resist those influences, and (c) correcting their perceptions of social norms.

The seminal study of this genre (Evans et al. 1978) relied heavily on McGuire's (1964) social inoculation theory. Social inoculation is analogous to biological inoculation, whereby a person is exposed to a small dose of an infectious agent in order to develop antibodies, thereby reducing susceptibility to subsequent exposure. This model, applied to smoking, posits that resistance to persuasion will be greater if one has developed arguments with which to counter social pressure to smoke (Evans 1976). According to the theory, the development of counterarguments should inoculate one against social influences in real-life situations in a manner analogous to biological inoculation increasing resistance to the disease inoculated against. Two studies by the Houston group (Evans et al. 1978, 1981) were based on this theoretical approach, with added theoretical bolstering from attitude change (persuasive communications) theory (McGuire 1969) and social learning theory (Bandura 1977). Their programs used same-age peers on film to impart information about the three major social influences to smoke and focused on immediate rather than long-term consequences of smoking.

Researchers at Stanford and Minnesota quickly followed the Houston research with a second generation of studies. They placed greater emphasis on social learning theory, and added elements derived from attribution theory (Jones et al. 1972) and commitment (Kiesler 1971). While the first studies by Evans et al. included films showing students being exposed to peer pressure and ways of resisting it, learning those skills does not appear to have been a primary

objective of the program. The second generation of studies allowed students to role-play and receive feedback on their performance of the behavioral skills. These relatively small-scale studies also included the first attempts to test (a) the contribution of various components of the programs, and (b) the differential effectiveness of different program providers or facilitators.

As this review will suggest, the so-called first and second generations of studies of the social influences approach to smoking prevention can now be considered as no more than pilot studies of a promising approach. They each had serious methodological flaws, including only one school per condition in the second generation studies, that made the interpretation of their results difficult. However, the consistency of their results made further exploration of the approach seem worthwhile. The Stanford and Minnesota researchers quickly moved into improved third generation studies, assigning two or three schools per condition using some form of randomization. These researchers were also involved in third generation studies by three other groups that tested the social influences approach in the context of larger community studies (North Karelia, Oslo, and Minnesota).

Third generation studies, while an improvement upon first and second generation studies, still had many methodological problems and, therefore, plausible alternative explanations of their findings. Smoking prevention research is a young field that is just beginning to mature. Early results, as in any new science, need to be considered only as suggestive, not definitive; and these early studies contributed more by the discovery of issues and the development of methodologies than by providing unambiguous results. Much improved fourth generation studies were made possible by the experiences and lessons of the early first, second, and third generation studies. Six fourth generation studies mostly have been large-scale randomized trials that have attempted to maximize internal validity in order to demonstrate the overall effectiveness of the social influences approach.

Seventeen school-based studies of the social influences approach to smoking prevention were located. [Studies that include broader life and/or social skills training (e.g., Botvin and Eng 1980, 1982; Botvin et al. 1980, 1983, in press; Pentz 1983; Schinke and Blythe 1981; Schinke and Gilchrist 1983) are reviewed by Botvin and Wills, and by Glasgow and McCaul elsewhere in this volume.] The 17 studies are reviewed in rough chronological order of their intervention within the four groups (generations) described above. For most of the studies to be reviewed, the date the study commenced corresponds with placement into the four generations. For each study the program(s) tested are described in terms of the grade(s) of intervention, number of sessions, total duration of the program, primary provider, types of peer leaders used, and other salient factors. Table 1 presents summary information about program characteristics. Methodological characteristics of each study are also described, including the experimental comparisons attempted, the number of units assigned to each condition, what those units were,

whether they were randomly assigned, whether pretest differences were reported, the time of longest followup, whether or not individual students could be tracked over time, the extent of attrition, and whether or not and the type of biological validation of self reports of smoking. Table 2 presents the summary of methodological information. The reported results are also described for each study, with comments on any plausible alternative interpretations, and these are summarized in table 3. Finally, I will synthesize the extent of our knowledge from all reviewed studies and provide recommendations for future research.

THE HOUSTON STUDIES (THE FIRST GENERATION)

Richard Evans and his colleagues (1978, 1981) at the University of Houston developed and tested the first of the social influences programs for smoking prevention. Their program used nonsmoking, same-age peers on film to impart information about the three major social influences to start smoking. The presentation of each film was followed by "knowledge tests" that emphasized immediate rather than long-term consequences of smoking, small group discussion of resistance to persuasion, and the provision of posters to be placed around the school to serve as continuing reminders. An experimental test compared students in schools that received this program over four consecutive days, a program plus feedback group that received both the program and feedback about the smoking rates among their classmates at the three posttests (at 1- 5- 10-weeks), a repeated testing group that was exposed only to the pretest and three posttests, and a minimal-testing control group that was exposed only to the pretest and the final posttest. A total of 750 students in 10 junior high schools were included in the study. Two schools were assigned to each of the four conditions (procedure unknown), and in two other schools students were assigned randomly to the four conditions. Reported results did not separate the between-school and within-school conditions. This was the first prevention study to include collection of samples of saliva to enhance the honesty of self-reports of smoking (Evans et al. 1977) by using a variation of the bogus pipeline technique (Jones and Sigall 1971). Results at the 10-week posttest indicated that the proportion of pretest nonsmokers in the program conditions who reported smoking at least one cigarette in the last month (10.0% in the program condition and 8.6% in the program plus feedback condition) was approximately half that of the minimal-testing group (18.3%). However, there were no significant differences between the program groups and the repeated testing group (10.3%). Note too that any subjects who reported having tried smoking at the pretest (31% of the total sample) were excluded from the analysis of program effects.

Evans and colleagues (1981) reported long-term results of the Houston program. A total of 13 schools were assigned (nonrandomly) to one of three experimental or four control conditions during the course of the study. Initially, however, a six-group design, three experimental and three control groups, was set up in seven of the schools. Aspects of the program (up to eight films presenting various messages) were provided to a cohort in each of the

experimental schools during each of three years, grades 7 through 9. The control schools varied from a repeated measurement condition (12 measures during the study that included saliva thiocyanate testing) to an additional set of schools that was added at the end of the study as posttest-only controls. Due to administrative and other difficulties, groups were successively combined during the course of the study so that only three conditions remained by grade 9. Subjects were not identified across time, so that only cross-sectional analyses were possible, rather than more appropriate longitudinal analyses. The reported cross-sectional analyses are rather difficult to interpret because the extent to which the composition of the sample changed over time is not known (sample sizes ranged from 1,352 to 3,296). It is clear that there were no program effects by the end of grade 7; indeed, there may have been a significant reverse effect. The authors claim significant program effects in subsequent years on the basis of approximately 9.5% of program students versus 11 to 14% of controls reporting smoking two or more cigarettes per day by the end of grade 9. Evans et al. also reported superior knowledge by the program groups and a correlation between knowledge and smoking behavior; the inability to conduct longitudinal analyses, however, means that such data cannot be interpreted to mean that changes in knowledge caused changes in behavior. Overall, it must be concluded that (a) this study did not replicate the earlier results at the end of the grade of intervention (grade 7), and (b) the claimed program effects at 3-year followup are small and difficult to attribute to the program.

THE SECOND GENERATION STUDIES

Despite the inconclusive results of the Houston studies, the theoretical derivations seemed firm enough to encourage other researchers to strengthen and test the approach further. The second generation of studies on the social influences approach to smoking prevention are characterized in three ways. First, they expanded upon the basic inoculation-with-communication model by enhancing the role of social learning theory and also considering attribution and commitment theories. Second, they attempted to test the relative contribution of the various components of their more complex interventions and/or the importance of the identity of the provider of each program. Third, they were relatively small-scale studies, as far as school-based studies go, with one school per condition.

Project CLASP (Stanford)

Investigators at Stanford (Perry et al. 1980; McAlister et al. 1980; Telch et al. 1982; see also McAlister et al. 1979) expanded upon the basic social inoculation with persuasive communication model. Project CLASP (Counseling Leadership Against Smoking Pressures) included the same features as the Evans et al. program with three important theory-based innovations. First, high school students were used as "peer" teachers for 7th graders; second, a session was introduced to increase social commitment not to smoke; and third, behavioral learning techniques (Bandura 1977) were introduced in the form of role-playing where students acted out situations requiring

resistance to social influences. The program consisted of three sessions on consecutive days, with four booster sessions spaced over the remainder of the grade 7 school year. The program was tested in one school that had been identified as having a high rate of smoking among older students and where administrators were seeking a solution to the smoking problem; two "roughly matched" schools were used as nonrandom controls. Breath samples were collected from all subjects in an attempt to increase the honesty of self-reports of smoking behavior. Individual subjects could not be followed, so analyses were limited to cross-sectional comparisons. At the end of grade 7, students in the treatment school reported significantly less smoking in the past week (5.3%) than students in the control schools (11%). At the end of grade 8, 5.6% of students in the program school reported smoking in the last week compared to 16.2% in one of the control schools (one control school was dropped from the analyses because of problematic pretest differences; McAlister et al. 1980). At the end of grade 9, 5.2% of students in the program school reported smoking in the last week, compared to 15.1% in the remaining control school (Telch et al. 1982).

This is the first study to have reported large preventive effects of the social influences approach, with the program group smoking weekly at only one-third the level of the control group. This is also one of the few social influences smoking prevention programs for which prevention of alcohol and marijuana use has also been reported (McAlister et al. 1980). Unfortunately, the encouraging results cannot be attributed totally to the program with any confidence because of the methodological problems noted above. The authors of the study are well aware of the shortcomings, and they label the study as a pilot project, pointing out some of the possible alternative interpretations:

It is possible that these results are biased by natural differences between the students in the two schools, by statistical regression, or by 'pseudo-regression' caused by deliberately choosing a population with reportedly acute problems as the experimental group and one with fewer reported problems as the control group (McAlister et al. 1980, pp. 720-721).

The First Minnesota Study (RASP)

The Robbinsdale Anti-Smoking Project (RASP) was initiated in the Fall of 1977. Data have been reported for the end of the intervention year (Hurd et al. 1980) and for 1- and 2-year followups (Luepker et al. 1983). Project RASP was the first study to attempt to test the value of (a) same-age peer leaders, and (b) a public commitment procedure. The initial design involved four schools and five experimental conditions. The five experimental conditions were (a) controls with questionnaire and saliva sample monitoring, (b) minimally measured controls (later dropped from the study because saliva samples were not collected at pretest), (c) social influences curriculum, (d) social influences curriculum with peer leaders (personalization), and (e) social influences curriculum with peer

leaders and a commitment procedure. The tested social influences curriculum consisted of a combination of video/film presentations (some of which were modifications of the Evans et al. materials) and discussion groups. The group discussions covered the social consequences of smoking, ways of saying no, correcting misperceptions of the proportions of people who smoke, and media pressures. Students were provided with opportunities to develop counterarguments, and role-play and practice resistance skills. In the peer-led condition, selected peers appeared in some of the video materials and led the group discussions. In the commitment procedure, students were recorded and played back to the class making a statement of why they were not going to smoke. The basic curriculum was four sessions long, with the commitment procedure adding a fifth period; the sessions were spread fairly evenly across the grade 7 school year, and were delivered or supervised by trained pharmacy students.

Classrooms within one school were assigned randomly to whether or not they received the commitment procedure. The four schools were assigned to the remaining four conditions so that one lower SES and one higher socioeconomic status (SES) school was assigned to program conditions and one pair to control conditions. All students (except those in the dropped minimal measurement conditions) were measured at pretest, between the second and third session of the curriculum, at the end of the school year, and at 1- and 2-year followups. A distinct improvement over previous studies was that individual students were tracked through the study.

At each measurement point, data were collected from approximately 80% of those present at the previous measurement point; 53% of the original students completed the 2-year followup. At the end of the intervention year, both the social influences programs seem to have reduced the amount of ever smoking, though not to a statistically significant degree. Only the social influences curriculum without peer leaders (but still with films) seems to have reduced experimental smoking; and only the peer-led social influences curriculum seems to have prevented an increase in regular (usually weekly) smoking. The commitment procedure did not add significantly to the peer-led program. By the 1-year followup, the only significant effect was a greater number of never smokers remaining in the peer-led condition school. By the 2-year followup, even that difference seems to have decayed somewhat, though when measures of quantity and frequency are included in a continuous index of smoking, the peer-led curriculum still appears to be superior to a significant degree.

This study made some innovative advances. Unfortunately, it can be considered only as a pilot study in that methodological problems make the reported results very difficult to interpret. The major difficulty is that SES and other social risk factors for smoking (e.g., parental, siblings, and friends smoking) were perfectly confounded with the expected strength of the experimental condition. That is, the control school was the lowest on SES and the highest on all the social risk factors; the school that received the social

influences curriculum with peer leaders was the highest on SES and the lowest on all social risk factors; and the school that received the social influences curriculum from pharmacy students was in between (see detailed pretest data reported by Hurd et al. 1980). Thus, without any program, the highest rates of smoking would be expected in the control school, and the lowest rates in the peer-led program school. Large pretest differences in smoking rates add to the difficulties of interpretation--at the final posttest, the rank-ordering of the three conditions remaining at that time in terms of level of smoking (experimental plus regular) is exactly the same as at pretest. Even the finding that peer leadership was superior is difficult to interpret, in that the selected peers also appeared in some of the film materials--that is, peer leadership was perfectly confounded with the familiarity of actors in the media materials.

THE THIRD GENERATION STUDIES

The second generation pilot research prepared the Stanford and Minnesota researchers to conduct larger and improved studies. The Minnesota researchers conducted an improved test of their program with grade 7 students. Both groups of researchers tested their approach with high school students. Individual investigators from these groups were also involved with three other groups of researchers who tested the social influences approach to smoking prevention in the context of larger community interventions. The seven third generation studies are reviewed below in chronological order of their starting date.

The North Karelia Youth Project

The North Karelia Youth Project (Vartiainen et al. 1983) consisted of community- and school-based interventions to influence behaviors that are risk factors for cardiovascular disease. A portion of the comprehensive program was a school-based smoking prevention curriculum that was based on the CLASP model (Perry et al. 1980).

Two selected schools in North Karelia comprised an intensive intervention condition in which project staff provided a 10-session curriculum; two matched schools in North Karelia represented a county-wide condition, in which regular teachers provided a 5-session version of the curriculum; and two matched schools in another county comprised a control condition. Grade 7 students were surveyed by questionnaire and serum samples at pretest (fall 1978), immediately after the 2-year intervention (fall 1980), and after a further 6 months (spring, 1981). Of all students participating in the pretest, 95% participated in the immediate posttest 2 years later, and 88% in the final followup 6 months later.

At the first followup, 21% of students in the intensive intervention condition reported smoking monthly, as did 19% in the county-wide intervention schools, compared to 29% in the reference schools. By the final posttest, these percentages had increased to 24%, 22%, and 34%, respectively. This pattern of results was repeated for proportions of students smoking daily as well. Analyses by gender

found that the significant program effects were confined to boys.

The reported results are very encouraging. However, they must be accepted with caution for at least two reasons. First, the method of school selection leaves open the possibility of school-level differences being responsible for the observed differences; however, the magnitude of the observed effects relative to the magnitude of pretest differences makes this unlikely. More serious is that the tested program was only one very small component, not only of a more comprehensive school-based program, but also of a large and very intensive community-wide intervention (Puska et al. 1981a, 1981b). It is, therefore, impossible to determine the extent to which the school program contributed to any overall effect; the lack of any differences between the intensive and county-wide conditions is inclusive.

The Second Minnesota Study (PCSC)

The second Minnesota project (Arkin et al. 1981; Murray et al. 1980, in press) overcame many of the methodological problems of Project RASP, and also attempted to make other advances in our knowledge of prevention. In two studies, three versions of a social influences curriculum were compared with a long-term influences (health) curriculum. Type of leader (adult health educator versus same-age peer), the use versus nonuse of media (films), and the public commitment procedure were also tested. In sequential replications of the study, type of provider/facilitator (research staff health educator versus regular classroom teacher) was tested. All curricula consisted of five class sessions for grade 7 students spread throughout the school year.

Eight schools were split at the median for pretest smoking rates, and then one from each group was assigned randomly to the four conditions. Within all eight schools, half the classrooms were randomly assigned to the commitment procedure. Two of the eight schools were control schools in Project RASP, and historical control data from them were used as the only control for the first study (research staff delivered). For the second study (teacher delivered), two additional schools, nonrandomized, served as a nonequivalent control group. Students were assessed by questionnaire and saliva sample at pretest (beginning of grade 7, in 1979 for the first study and in 1980 for the second study), immediate posttest at the end of grade 7, and 1-year followup (for the first study only) at the end of grade 8. Most results were reported in terms of an index of weekly smoking formed by averaging three self-report measures of smoking. Results are reported only for pretest nonsmokers and experimental smokers. The few students who were regular smokers (i.e., at least monthly) at pretest were not included in analyses of program effects.

Data reported by Arkin et al. (1981) suggest that in the short-term, the health consequences program was most successful in reducing the proportion of nonsmokers who tried smoking by the immediate posttest (15%), and the social influences programs were next most successful

(18-21%), when compared with the historical control (31%). The analyses presented by Murray et al. (in press), based on the composite index measure of smoking and covarying for pretest differences in social risks to smoke (i.e., parental, peer, and sibling smoking and SES), did not show differences between the four program conditions but did suggest that all four programs were better than controls. By 1-year followup of the first study, however, the peer-led conditions appeared to be more successful for pretest nonsmokers. When pretest experimental smokers are considered, there were no differences between conditions when health educators delivered all programs (Study I); but the peer-led social influences (also called short-term influences) program was more effective by the immediate posttest when delivered by teachers (Study II). However, the pretest experimenters in the control group had the lowest level of smoking by the immediate posttest, making it difficult to interpret any other program differences. The pattern of differences found among the four program conditions at the immediate posttest for Study II was replicated at 1-year followup in Study I. That is, by 1-year followup, pretest experimental smokers were smoking at significantly higher rates when they received the social influences program without peer leaders than when they received either the social influences program with peer leaders or the health program from health educators. At no point in time, in either study, were there differences due to the use of films or the commitment procedure.

These results are difficult to interpret for several reasons. First, despite the use of an improved procedure of random assignment after matching, baseline smoking experience in Study I was lower for the two peer-led conditions (average proportion never smoked = 58%) than for all other conditions (average of 64% for the other program conditions, and 56% for the historical controls) (Arkin et al. 1981). Second, no baseline smoking level data were reported for Study II--the existence of large differences in the same schools in Study I would lead one to expect such differences in Study II unless otherwise reported. Third, pretest differences in smoking levels were not adjusted out, even though the acknowledged (but unreported) baseline differences in social risk factors were covaried out in all but the Arkin et al. (1981) paper. Fourth, the use of historical and nonequivalent control groups is questionable, and no baseline data were reported for one of them.

To summarize, these studies attempted to overcome many of the problems of Project RASP, and found (a) no significant overall differences between programs at the immediate posttest, (b) a suggestion that social influences programs were more effective when peer leaders were used, (c) that peer-led social influences programs were more effective than adult social influences or health programs at preventing pretest nonsmokers from starting to smoke, and (d) no significant program effects on pretest experimental smokers. However, despite the methodological improvements, the meaning of any of these findings is unclear because of serious methodological problems that remained.

The Oslo Youth Study

Tell et al. (in press) report results from a test of a social influences smoking prevention program delivered as a component of a comprehensive health education curriculum. A 10-session program was delivered to grades 5, 6, and 7 students (age 11 to 14) partly by older peer leaders and partly by project staff. Six schools were formed into matched pairs. One school from each of two pairs was randomly assigned to receive the program, while the third program school was assigned due to an existing relationship between that school and project staff. Signed consent for participation was obtained from 82% of parents. Students were pretested via questionnaire, saliva samples, and other health measures at pretest (early 1979), and 2 years later (early 1981). The intervention took place over the 2 years between surveys. Sixty-eight percent of the initial sample of students completed both questionnaires. Attrition was greater in control (40%) schools than program schools (25%).

Overall results indicated that the program had an impact upon those students who had never tried smoking prior to pretest. Of all pretest students who had never smoked, 16.5% of the program group and 26.9% of the control group reported smoking by the posttest. In a step-wise discriminant analysis, the program entered after pretest measures of "acceptability of smoking," parental concern with the student's health, gender, and availability of discretionary funds. Pretest measures of friends' smoking and smoking knowledge also entered at significant levels after the effect of the program. Significant program effects were also reported for smoking knowledge and intentions. Similarly, changes were observed in other health-related behaviors as a result of the complete program--the program group improved significantly more than the control group in terms of exercise and alcohol consumption.

The results of the Oslo study are similar to others, with the pretest nonsmokers from the program group smoking at 39% less than the control group by the final posttest. However, several factors prevent clear attribution of the observed effects to the social influences smoking prevention curriculum. First, the smoking prevention curriculum was only a portion of a much more complex health behavior curriculum; the extent to which the remainder of the program, or any changes caused by it, led to the changes in smoking behavior cannot be determined. Second, there were reported pretest differences, with the program students being more knowledgeable and viewing smoking as being less acceptable, and being almost twice as likely to have tried alcohol. Third, there was differential attrition from program and control groups, although the direction of this difference (greater attrition from control schools) would tend to reduce the chances of finding significant program effects.

Current Minnesota Studies

As part of a large-scale, community-based heart disease prevention project (Blackburn et al., in press), several approaches to smoking prevention are being tested (Perry et al. 1983a). One of these was a

social influences program. A six-session version of the "Keep It Clean I" program was tested in three schools (N = 397) within one of the program communities in 1981-82. Two schools on the outskirts of Minneapolis were matched on size, SES and grade 7 smoking prevalence, and used as controls (N=325). The program was taught 1 day each month during the school year. Teachers and same-age peer leaders received special training from project staff. Students were assessed with questionnaires and saliva samples at the beginning and end of the school year.

Equal numbers of students (4.7%) in the program and control schools reported smoking in the week prior to pretest. At posttest, more control than program students reported smoking in the previous week (8% versus 5%); this difference was marginally significant. When all grade 7 and 8 students in both sets of schools were assessed, it was found that 8.1% of the program school students reported smoking in the previous week compared to 11.8% of control school students. This suggests that some or all of the observed effect could have been due to the other antismoking activities being conducted in the communities of the program schools. That is, whether the effects on smoking onset are attributable to the school-based program or "were complemented by the entire community-wide project cannot be determined by this design" (Perry et al. 1983a, p. 11).

The Stanford High School Studies

The Stanford group has tested the social influences approach with high school students (Perry et al. 1980b). The tested program emphasized the short-term physiological effects of smoking and the social pressures influencing adoption of the smoking habit. For the short-term physiological effects component, measures taken from smoking and nonsmoking students were compared. Students were also introduced to several smoking cessation procedures. Regular health teachers were trained to deliver the program on four consecutive days. All five high schools in one school district near Stanford University were matched on SES and then assigned randomly to experimental or control conditions. All students in three schools received the program, and students in the other two schools received the traditional tenth grade health material emphasizing the harmful long-term effects of smoking. Students were assessed by questionnaire and physiological measures at pretest (September 1978) and posttest (February 1979).

At posttest, fewer students in the program condition than in the control condition were smoking (in the past day, the past week, or the past month). These results appear to be statistically significant, and they were paralleled by changes in biochemical indicators (carbon monoxide) and knowledge. This is the first study to demonstrate that the social influences approach can be effective for high school students. Unfortunately, separate results were not reported for those students who were smokers at pretest, and no long-term data are available.

In a second high school study, Perry et al. (1983b) used a 2x3

factorial design to compare the relative effectiveness of teachers and college-age peer leaders in delivering health versus social influences versus physiological effects programs. Twenty classrooms from four high schools were randomly assigned to the three levels of programs; then the four schools were randomly assigned to teacher or college student delivery. Each program consisted of three 1-hour sessions. Students were pretested immediately prior to the programs (February 1980) and posttested about 2 months after the programs (May, 1980). Data for assessments were obtained from questionnaires and carbon monoxide samples. Staff members observed the implementation of the programs in all classrooms.

All programs were implemented equally well. Of all students who reported weekly smoking at pretest (N=82), 23% reported not smoking during the week prior to posttest. Any differences between conditions were not statistically significant because of small sample sizes, and there certainly were no significant changes in the overall rates of weekly smoking. (The apparent effects observed, however, suggested that teachers may have been better than college students at delivering health information, but college students may have been better than teachers at delivering the social influences program--this pattern was also observed in the second Minnesota study [PCSC].)

The USC High School Study

The High School Anti-Smoking Project (NASP) at the University of Southern California (Johnson et al. 1984) compared the relative effectiveness of social influences and health programs with grade 10 students. The value of peer leaders and the differential contribution made by having familiar peers as actors in the media material were also tested.

The tested social influences program included material on media influences, social influences and skills for resisting them, values clarification/decision making, and a public commitment procedure. The health program included materials on lifestyles and health, the long-term effects of smoking, the short-term physiological effects of smoking, and the public commitment procedure. Both programs included four sessions delivered by project staff (health educators) over a 3-month period.

On the basis of pretest data, nine high schools were stratified and eight of them formed into pairs such that one high- and one low-smoking school were yoked together. The four yoked pairs were then randomly assigned to four experimental conditions created by crossing the two program types (social versus health) and whether peers in media material were familiar or unfamiliar. The one remaining unyoked school, which was ranked in the middle for smoking level, was assigned to be the control. Within each program school, the program was delivered to only that half of the student body who were in health classes during the semester of intervention. The other half acted as within-school controls. One half of the classes

that received the program were randomly assigned to a peer leader condition. The other half received the program without the use of peer leaders. Students were assessed via questionnaire and saliva samples at pretest (January/February 1981), immediate posttest (May/June 1981), and at 1- and 2-year followups (May 1982 and 1983). All data collection was conducted by trained data collectors who visited the schools on predetermined, but unannounced, days. In most cases, the classes in which data were collected were not the same classes in which the program was or had been delivered. Attrition rates were very high--65% by the end of the study, with no differences in rates between conditions (Hansen et al. in press).

Three sets of main-effect comparisons were made--social versus health program, peer leaders versus no peer leaders, and familiar versus unfamiliar actors in media material. Possible interactions were ignored because of limited sample size. Comparisons were made for four possible transitions in smoking behavior: no smoking to any smoking, experimental use (monthly or less) to heavier use, regular use (a pack or less a week) to heavy use (more than a pack a week), and current smoking (regular or heavy use) to nonsmoking (quit) status.

Attrition was lowest for pretest never smokers (28% by the final posttest). Pretest never smokers were marginally less likely to become users if they were exposed to the health program than if they were exposed to the social influences program or if they were controls. Significant differences were observed only at the 1-year followup, where 46% of the health program students had tried smoking versus 59% of the social program students and 58% of controls. Although not significant, the same general pattern held at the 2-year followup (60% versus 68% versus 68%, and 65% for the within-health-school controls).

Of all experimental smokers at pretest, only 51% were present at the immediate posttest, 36% at the 1-year followup, and 31% at the 2-year followup. Using these samples, significant differences in the onset of regular smoking was observed, with the social program holding the onset rate to approximately half that observed for the health program and control students (4% at immediate posttest, to 6% at one year, to 7% at 2 years for social program students, versus 8 to 9% to 9% to 13 to 14% for both health program and control students). When the 1- and 2-year followup waves were combined, attrition was significantly less than when considering either wave on its own (59% of subjects had data at pretest and at least one of these followups). The above pattern of results was replicated using these pooled data, possibly reducing, to a small extent, the concern raised by high levels of attrition. Attrition by pretest regular smokers was very high (87% by the 2-year followup), and no significant program effects were detectable using the remaining sample. Neither peer leadership nor familiarity of actors in media materials made any significant difference to the pattern of any of the results.

The reported results are obviously compromised to an unknown extent by (a) severe attrition, and (b) possible ceiling effects, where only

a small proportion of the total sample had never tried smoking even at pretest.

SUMMARY OF SECOND AND THIRD GENERATION STUDIES

The nine second and third generation studies have all provided encouraging results. While results from no one of the studies were interpretable in their own right because of methodological problems, the consistency of findings is impressive. However, an analysis of the methodological issues is worthwhile because if different studies have different methodological problems, then they ought to cancel each other out across studies, and the consistency of results would then be an indication of a robust finding. On the other hand, if one or more methodological problems, or threats to internal validity are present in most or all studies, then the consistency of results would not be very informative.

With only one to three schools per condition, often nonrandomly assigned, many studies had noncomparable pretest levels of smoking behavior and/or social risk values (i.e., numbers of peers, parents, or siblings who smoke, and SES). In one study (CLASP), the program school was identified as being a higher risk than the control school. The label of a "problem" school in this respect might indicate that additional "interventions" were going on at the same time as the smoking prevention program and as a result contribute to any program effect. However, the fact that the relative smoking rates in the two schools were reversed between pretest and posttest, and the magnitude of that effect, suggest that the smoking prevention program may well have been at least partially responsible. Such an interpretation must be considered with great caution, however, given that there was only one school per condition in this study so that unknown selection bias could still have been operating.

In contrast to CLASP, the two early Minnesota studies (RASP and PCSC) both had pretest differences such that the hypothesized strongest program group was at lowest risk to becoming smokers while the hypothesized weakest program or control groups were at highest risk. Thus, the observed patterns of results could have been observed even without any interventions. The fact that some effects remained after adjusting for some of the risk factors provides some encouragement. However, these analyses were not able to adjust for pretest levels of smoking (though they may well have been adjusted out, at least in part, with the social risk factors), and the shortcomings of such statistical adjustment are well known (Cook and Campbell 1979). Clearly, we must interpret results from studies with known pretest differences with great caution.

Three of the programs for which positive results were reported were but small parts of more comprehensive and intensive interventions, making it impossible to determine how much of the effect was due to the tested program rather than the overall intervention. Three of the studies provided tests of the social influences approach to smoking prevention for high school students. The results were rather mixed, however, and difficult to interpret, but they provide

encouragement for further research on the application of the social influences approach to high school students.

Attrition was also a very serious problem for most of these studies. Attrition can be a threat to internal validity if it is differential across conditions. This seemed to be the case in only one study (Oslo), and then in the direction that would be presumed to decrease the chances of detecting a program effect. External validity is always threatened by attrition, however; for example, if students at high risk of becoming smokers are more likely to be absent from measurement, this would decrease the possible program effect that could be observed. See Hansen et al. (in press) for an analysis of attrition issues in smoking prevention research.

Reviewing of many of these studies was often difficult because of inadequate reporting of data. Some did not provide pretest data, and most provided inadequate data about smoking behavior at posttests. As we have noted elsewhere (Flay et al. in press) researchers should report results for all categories, as established in pretest measures, of smoking behavior and not just selected significant findings, to enable readers to fairly judge the practical significance of findings and make comparisons across studies.

Reported results across these nine studies were not as consistent in their exact nature as in their magnitude. RASP, PCSC, Oslo, Stanford High I, and the HASP health program were reported as reducing the onset of smoking by pretest nonsmokers. CLASP, North Karelia, Current Minnesota, and the HASP social program, on the other hand, were reported as reducing the prevalence of regular smoking and/or the transition from experimental to regular smoking status. In most instances, the results for the other type of change were not reported.

All of the second and third generation studies are also susceptible to two other alternative interpretations of any observed effects:

- (a) a testing by treatment interaction, because in most instances the program and data-collection activities would have been perceived by students as related; and
- (b) a Hawthorne effect, in that program students received more special attention than controls in almost all studies.

The Stanford, Minnesota, and USC studies attempted to test the value of various components of the social influences approach, particularly the use of peer leaders, social versus health programs, the use of media, and the use of a commitment procedure. However, given the generally low internal validity and uninterpretable nature of these studies, none of the findings from these comparisons can be accepted wholeheartedly without further replication.

On the basis of the earlier reports from the second and third generation of studies, Fisher (1988) concluded that "It now seems well established that these interventions work, at least to an appreciable if not totally satisfactory extent" (p. 678). While

these studies, taken together, provided some encouragement for examining the social influences approach further, the numerous methodological problems with most of these studies when considered alone, and the fact that some of the problems were common to all studies, do not allow us to agree with Fisher. Indeed, the reported results evidently were not considered by most researchers to be robust enough to warrant large investments in large-scale studies designed to test components. Rather, most of the fourth generation of studies were designed with the sole objective of establishing whether or not the social influences approach to smoking prevention is efficacious when tested under more rigorous methodological conditions.

THE FOURTH GENERATION STUDIES

Despite the many weaknesses of the above studies, the consistency of reported results provided the impetus for improved studies. The fourth generation of studies on the social influences approach to smoking prevention placed a primary value on enhanced internal validity. They may be characterized as large-scale field trials, with five or more (half with 11 or more) units randomly assigned to each condition. Most may also be characterized as demonstration projects or summative evaluations, in that they compared only program and control conditions, without attempting to test components or providers. The six studies reviewed in this section varied, however, in the success with which the tested programs were implemented, the exact nature and length of the program tested, the grade level on which it was tested, the type of peer leaders used, and who provided it. The latter variable was explicitly tested in one of the studies.

The Waterloo Study

The Waterloo Smoking Prevention Program (Best et al. 1984; Flay et al. 1983a, 1985) followed the basic principles of the social influences approach as improved at Stanford and Minnesota but added a component on decision making that was tailored to the smoking decision. The program was tested on grade 6 students, 1 year earlier than most other studies. Six one-hour, weekly sessions were delivered by health educators near the beginning of the grade six school year (October/November 1979); two maintenance sessions were delivered near the end of grade 6; and three booster sessions were provided, two at the beginning of grade 7 and one at the beginning of grade 8. Evans' films were reproduced for the Canadian context using student actors from the local theater group who were 1 or 2 years older than the grade 6 students. Live peer leaders were not used in the classroom, but the health educators were Master's students who deliberately "underdressed" and encouraged students to call them by their first names. Thus, they were similar to the college-age peer leaders used in the Houston studies and Project RASP.

Twenty-two schools in two school districts in Southern Ontario volunteered to participate in the study, and 11 were assigned, mostly randomly, to each of experimental and control conditions. Pretest differences were minimal, and were not observed for smoking behavior.

Study students were in the same schools for the duration of the project, grades 6 through 8. Students were tested with questionnaire and saliva samples at pretest (T1), immediate posttest (T2), the end of grade 6 (T3), the beginning and end of grade 7 (T4 and T5), and the end of grade 8 (T6--30 months after the core program). The health educators were present at pretest and immediate posttest data collection in both program and control schools, but new project staff collected data at all other followups. Total attrition plus absenteeism was less than 10% per year, and 67% of students provided data at all six data points.

Program results were analyzed according to the pretest status of subjects, and considerable complexity was found in the pattern of changes over time. For students who had never tried smoking prior to pretest, the program was marginally effective in preventing trying--by the end of grade 8, 53% of the control students who were never smokers at pretest had tried smoking, while only 40% of the program students had done so. For students who had tried smoking but classified themselves as quitters at pretest, 69.2% of the program group and 50% of the control group remained quitters at the end of grade 7, but this difference had reduced to 50% versus 46% respectively by the end of grade 8. For those students who had tried smoking only once prior to pretest, by the end of grade 8 almost equal proportions of the program and control students had tried smoking again (64% versus 63%). However, significantly more program than control students had decided to never smoke again (43% versus 25%), and only 5% of the program group versus 13% of the control group had become regular smokers (usually every week). For students classified as experimenters (tried more than once, but smoke less than weekly) at pretest, the results varied more across time. Almost all of the control students in this category (95%) reported smoking on at least one of the five posttests, compared to less than three quarters (74%) of program students. A high proportion of program group experimenters quit immediately following the program, and this effect held to the end of grade 7, when 63% of this group were still quitters compared with 28% of the control group experimenters. By the end of grade 8, however, so many more of the control group experimenters had also quit that the difference was no longer significant (58% of program group versus 50% of control group). (Note that we expect many experimental smokers to quit eventually because many more adolescents experiment with smoking than ever become regular smokers). The small number of pretest regular smokers (five in the program and eight in the control conditions) precluded any statistical analysis of program impact on this group. In summary, program effects varied over time, having greatest immediate effects on those experienced with smoking and later effects on those with no or little smoking experience at pretest.

The Waterloo investigators also analyzed the effects of their program on those students assumed to be at high risk of becoming smokers for social influence reasons. The program was found to have its major effect on those students who had parents, siblings, and friends who smoke. For example, among high-risk students who had never tried smoking prior to pretest, by the end of grade 8, 67% of those in the

program group versus 22% of controls still had never smoked, 6% of the program group versus 39% of controls were experimental smokers, and none of the program students versus 6% of controls were regular smokers (Best et al. 1984). Such results provide some validation for the theoretical rationale for the social influences approach to smoking prevention.

The Waterloo study successfully overcame many of the methodological problems of earlier studies, so that the reported results are more readily interpreted as being due to the program. However, while pretest comparability was maximized, attrition minimized, and more detailed analyses reported, there remain several plausible alternative explanations of at least some of the reported program effects. First, a measurement by treatment interaction is possible. This is especially likely at the first posttest where the health educators also collected the data--although the collection of saliva samples should have minimized it, and the lack of any significant program effects at that point suggest that this interpretation of subsequent effects is not very plausible. Second, as in all other studies to date, a Hawthorne effect may be operating--the program students received a great deal of attention from outsiders, and their subjective evaluations tell us that they liked the health educators a great deal. Third, there was large variability between schools (not yet reported in detail), and program effects were apparent in only some of the program schools--was there something special about them that caused some or all of the observed effects? This question cannot be answered. Despite these limitations, however, the Waterloo study represents one of the more rigorous tests to date of the social influences approach to smoking prevention; and the demonstration of strong program effects on those students most at risk provides strong support for the approach.

The Stanford/Harvard Study

McAlister et al. (1982) tested a 12-session, 2-year version of the CLASP curriculum delivered to grade 7 and 8 students by high school students under the supervision of research staff. Junior high or middle schools were randomly assigned to program or control conditions from five matched pairs in Massachusetts and California. The program was not implemented in full in two of the program schools because of administrative difficulties. Saliva samples and questionnaire data were collected from students on four occasions--October 1979, May 1980, October 1980, and May 1981. School districts insisted that individual students not be identified, so students generated their own I.D. codes. Analyses indicate some problems with inconsistent use of codes (McAlister 1983). School administrators estimated that between the first and last survey periods approximately 38% of the students transferred to other schools. Approximately 15% of parents excluded their children from measurement, with no differences between conditions within sites. Overall, only about one-third of the original students could be included in the longitudinal (cohort) analyses.

Overall results suggest that the program only marginally reduced the

rates of weekly or monthly smoking, regardless of whether cross-sectional (using all responses at each time point) or longitudinal (using only those students who responded and could be matched across all measurement periods) analyses are considered. Wide school variations were observed, however, and the authors attempt to provide reasons for significant program effects in two pairs of schools and not others. Unfortunately, in the two pairs of schools where significant program effects were observed, the program schools had over 60% higher rates of smoking prior to the program than the control schools. While the pattern of effects looks promising, with the proportion of smokers actually decreasing in the program schools and increasing in the control schools, it is difficult to interpret; it could be due to expected patterns of quitting by early starters as observed with grade 8 students in the Waterloo study. In those schools where pretest levels were more nearly equal, no significant program effects were observed. Significant program effects on marijuana use was reported for only one school pair--where there was a large pretest difference.

The Australian Study

Fisher et al. (1983) have tested the social influences approach to smoking prevention in Western Australia. The Minnesota program (Arkin et al. 1981) was modified only slightly, including the remaking of film materials, for the Australian context and idiom. The program was delivered to grade 7 students, one session every month over the last 5 months of the Australian academic year (August to December 1981). The study compared same-age peer- versus teacher-led programs and controls. Both teachers and peer leaders received special training for the program.

A total of 45 elementary schools were randomly assigned to the three conditions after stratification on school size, geographical location, and SES. Students were assessed with questionnaires and saliva samples immediately prior to the program, and 1 year after the end of the program (November 1982). The data collectors were blind to the experimental condition of the students. By careful tracking, 82% of the initial sample were posttested, even though students had made the transition from elementary to dispersed high schools during the intervening year.

One-year followup results indicated that teacher- and peer-led programs both reduced the onset of smoking among girls (26% of pretest nonsmokers had smoked during the 12 months prior to posttest in the two program groups compared to 35% for the control group--i.e., a 26% reduction in onset), but the effect was only marginally significant for the peer-led curriculum after adjustment for the effect of social risk factors. Only the teacher-led program was effective for boys (19% in the teacher-led condition, compared to 36% in the peer-led condition and 31% of controls--i.e., 39% reduction in onset). No program effects were observed for students who had smoked during the 12 months prior to the program.

This study demonstrates that a social influences classroom curriculum

can, by itself, be effective in countries other than the United States and Canada, at least in reducing onset by previous nonsmokers, but raises questions about the superiority of peer leaders over adult leaders. It appears that the use of peer leaders may sometimes not be beneficial. Of course, cultural differences could explain this finding--Australian adolescents may not identify as closely with their peers as Americans do. It also seems that the peer leaders had a greater responsibility for the entire program than same-age peer leaders have had in any other study, and one must wonder how well they were able to implement the program. Small pretest differences between conditions in smoking prevalence (nonsignificant) cannot explain the observed effects of the teacher-led programs, and a multiple logistic regression analysis demonstrates that social risk predictors of smoking (e.g., number of friends who smoke, response to cigarette advertising, intentions) do not explain them either.

The Michigan Study

Dielman et al. (1984a, 1984b) tested the social influences approach to smoking prevention on grades 5 and 6 students. The program consisted of four sessions delivered by health educators on the research staff. In addition, grade 5 students also received a three-session booster in grade 6. In a controlled study, 18 schools in Ann Arbor were formed into matched pairs on achievement scores, ethnic distribution, and SES, and then randomly assigned to program, control, or "mixed" conditions. In the mixed condition (two schools), classrooms were randomly assigned to program or control conditions. Signed consent was provided by 83% of the parents of program students and 75% of parents of control students. Students were assessed via questionnaire only (i.e., no biological samples were collected or bogus pipeline procedures used) at pretest (March 1981), immediate posttest (June 1981), 4-month followup at the beginning of the following academic year (October 1981), and 1-year followup (June, 1982). The health educators who delivered the program were usually present at data collection in both program and control schools. Approximately 88% of students were present at all four measures.

By the second and third posttests, significant effects were observed on recent smoking. Reports of smoking in the last month increased from 1% at pretest to 7% by the 4-month followup to 15% by the 1-year followup for control students, versus 4% to 1% to 8% by program students. No effects were observed on intentions to smoke in the future, or on the numbers of students who had ever tried smoking cigarettes (increase from 32% at pretest to 50% at 1-year followup for both groups). Alcohol and marijuana intentions and behavior were also measured, but no program effects were observed.

The lack of any biological sampling, plus the presence of the health educators in the classroom during data collection, makes interpretation of these data somewhat precarious, because both of these factors can serve to increase the demands on program students to underreport their smoking. However, the lack of any program

effects on either intentions or trying smoking suggests that these alternative explanations of the reported findings may not be entirely plausible. Because of the young age of the subjects, final evaluation of this study requires long-term followup data.

The Oregon Study

At the Oregon Research Institute, Biglan et al. (in preparation) tested a social influences program delivered/facilitated by classroom teachers. The tested program consisted of three sessions delivered on consecutive days with one booster session delivered about 2 weeks later. Grades 7 and 9 classes of volunteer teachers in six middle schools and three high schools were randomly assigned to program (N=41) or control (N=45) (traditional health education) conditions. Students were assessed by questionnaire and saliva sample at pretest, 6-month posttest, and 12-month posttest. Seventy-seven percent of the initial students were present at the 6-month assessment and 68% at the 12-month assessment. The questionnaire included innovative measures of the proportion of cigarette offers refused during the week prior to measurement. In addition, refusal skills were assessed directly for a sample of students (Hops et al. 1984).

Despite random assignment, control students were significantly more likely to have smoked prior to pretest. Accordingly, analyses of covariance procedures were used in tests of program effects. At the first posttest there were fewer regular smokers (at least weekly) among the program than the control students (10% versus 14%). By the 1-year followup, however, this marginally significant difference had disappeared (10% versus 11%). When only those students who were smokers prior to pretest were considered, there was still a significant program effect at 1-year posttest, but only for relatively heavy daily smoking (more than 10 cigarettes every day). Program effects were evident on both the questionnaire and behavioral measures of use of refusal skills. Students present at pretest but not at subsequent tests were more likely to be smokers and to have parents who smoke. They also scored significantly higher on a scale of deviant behaviors involving alcohol and marijuana use, and had lower educational aspirations and less educated parents.

The small magnitude of effects on behavior in this study is somewhat intriguing, given the demonstrated effects on presumed intervening constructs. The assignment of classrooms could have reduced the difference between treatment and control conditions, in that one of the presumed effects of the social influences programs is thought to be alteration of the norms for complete social environments. The demonstrated differences between groups in the use of social resistance skills reduces the plausibility of this interpretation, but does not remove it entirely. Other data that might throw light on this issue have not yet been reported. Fairly serious attrition also raises questions in that those students who were most likely to become smokers were less likely to be present for followup measurement--thus reducing the probability of being able to detect program effects that might have occurred. On the other hand, the reported pretest differences would lead one to expect the observed

cost of its procurement. Best and Pechacek (personal communication 1983) have suggested a procedure that could reduce the cost of this assessment by pooling some of the saliva from a number of subjects who have indicated on questionnaires that they do not smoke. Validation of cotinine using this procedure has not been reported to date.

The Detection of Low Rate Smoking

While studies of the validity of smoking measures are encouraging, it should be noted that none of these measures assess the type of low-rate smoking that has been the target of most prevention studies. For example, a number of studies have reported beneficial effects of programs on self-reported experimentation with cigarettes (e.g. Botvin and Eng 1982). To date, evidence does not demonstrate that such low levels of smoking can be accurately detected. Similarly, demonstrations that physiological assessments increase self-reported smoking have not specifically assessed whether the accuracy of reports of experimentation is increased. Thus, one cannot infer the validity of measures of self-reported experimentation on the basis of the extant evidence. For this reason, we cannot be as confident about the validity of studies which show the prevention of experimentation as we can about studies showing the prevention of higher levels of smoking.

The Importance of Studying Endpoint Smoking

The last point underscores the importance of examining the effects of prevention programs on "endpoint" smoking. The concept was first suggested by Terry Pechacek (personal communication 1983). Endpoint smoking might reasonably be defined as the presence of high-rate smoking at the end of the high school career. Further, "high-rate" smoking might be defined as that level of daily smoking that is so high that it is very unlikely that the person will stop smoking. Only further research will determine that level. The ultimate question for smoking prevention programs is whether they prevent endpoint smoking. Demonstrating an effect on endpoint smoking would increase confidence that our prevention programs are actually contributing to a reduction in the risks of morbidity and mortality that are associated with smoking.

None of the studies to date have assessed the impact of programs on endpoint smoking. This statement is not a criticism of those studies, since it would have been premature to invest the large sums of money needed to examine such an impact prior to the existence of some evidence that adolescent smoking might be preventable. But, the very successes of those programs make it timely to inquire about likely effects of our programs on endpoint smoking.

Current successes may not necessarily translate into an effect on endpoint smoking. There is evidence that large numbers of young people (as many as 29.8%) smoke cigarettes regularly and quit at some later point (Johnson 1982). In our own work we have found a significantly lower smoking rate among adolescents that stop smoking six months later, $F(1,79) = 12.52$, $p < .0001$) and one year later, $F(1,61) = 5.14$, $p < .027$) than among adolescents that continue to smoke. Moreover, as table 2 shows, the smoking rate of smokers in seventh grade is significantly lower than it is for those in upper grades. Thus, studies that show that low-rate smoking of seventh graders has been prevented do not necessarily mean that endpoint smoking will be deterred. At the same time, there is evidence that measures of experimental smoking do predict later higher-rate smoking (Ary et al. 1983). Thus, the deterrence of experimentation and low-rate daily smoking probably has some value for reducing endpoint smoking.

TABLE 2

Number of Self-Reported Cigarettes Smoked by Grade Level

Grade	Number of Smokers	Mean Number of Cigarettes in Past Week
7	52	16.4
9	42	38.6
10	20	48.6
11 & 12	29	42.4

Note: Difference among grades, $F(3,135) = 3.76$, $p = .012$. Grades 7 and 10 differ significantly according to a Tukey HSD test.

These considerations point to several important requirements for further research on smoking prevention. First, follow-up of subjects, at least through their high school careers, is needed to document that endpoint smoking is indeed affected. As we discuss below, the serious problem of subject attrition will need to be overcome in order for such follow-up to be

meaningful. Second, studies are needed which focus on the transitions from lower levels of smoking to higher levels. What is the prognostic significance of a given rate of smoking or a given topography for endpoint smoking, and what factors affect these transitions? Third, the impact of our programs needs to be examined separately for distinct categories of adolescent smoking status. For example, we should not assume that preventing those who have never smoked from beginning to experiment implies that current experimenters have been prevented from becoming regular smokers. To some extent this is being done now. For example, Flay et al. (1983) looked at the effects of their smoking prevention program on each of the following groups: a) those who had never smoked, b) those who reported having smoked but had quit, c) those who reported experimenting, but smoked less than one cigarette per week, and d) regular smokers, defined by self-reports of smoking at least once a week. As studies of the transitions to regular smoking accumulate, the practical significance of effects on each of these groups will become clearer.

The Value of Studying Adolescent Cessation

There may be particular benefit in studying cessation among adolescents who are already smoking regularly. Some of the success of prevention programs may be due to their prompting smokers to quit. At least two prevention programs have shown effects on students who were already smoking regularly at pretest assessment (Biglan et al. 1985; Flay et al. 1983; Ryan et al. 1983). While other prevention programs may have affected those subjects who were already regular smokers, it is difficult to be certain, because the analyses combined subjects who were smoking regularly at pretest with those who were not (cf. Evans et al. 1983). Regular adolescent smokers often desire to quit (Biglan & Lichtenstein in press) but they encounter continuing social influences to smoke (Kniskern et al. 1983; Biglan et al. in press). Skill in refusing cigarettes could thus facilitate cessation for those who are already smoking.

These considerations suggest an alternative research strategy. Regular smokers might be identified and randomly assigned to programs like those of Flay et al. (1983) and Biglan et al. (in preparation) or to control conditions. There are at least three advantages to such a research strategy. First, this group of smoking adolescents has the highest risk of emerging from their high school careers as regular smokers (Ary et al. 1983); deterrence of their smoking is of paramount importance. Second, such a strategy would allow random assignment of individuals rather than whole schools to conditions, thus avoiding some of the vexing statistical power problems we just described. Third, the cost of such studies would be far lower

than the massive multischool evaluations of prevention programs.

ATTRITION: A THREAT TO EXTERNAL AND INTERNAL VALIDITY

Attrition rates in studies of smoking prevention have been relatively high. For example, Murray et al. (1983) reported a 10.7% attrition rate among junior high school students at nine months and a 28.9% attrition rate at a 21-month follow-up. In the study we conducted (Biglan et al. 1985), 17.5% of middle school students had dropped out at 6 months and 25% were missing at one year. For subjects in high school the rates were 17.9% at six months and 38% at one year.

External Validity

Hansen et al. (1983) have called attention to the threats to internal and external validity that subject attrition poses to evaluations of smoking prevention programs. They suggest two questions regarding the possible effects of attrition on external validity. First, they ask whether pretest scores for dropouts differ from those of subjects who stayed in the program. Second, when assessments are available from subjects from the first two of three assessments, they ask whether the pattern of change in use of substances for those who have dropped out by Time 3 differ from the patterns of those who have remained. They examined these issues for two studies of cigarette and drug prevention that had been conducted by the Health Behavior Research Institute. For both projects they found that students whose data were missing at posttest and at one-year follow-up were significantly more likely to have reported use of cigarettes, alcohol, and marijuana at Time 1, with the exception of alcohol consumption at one-year follow-up in one study. With respect to the second question, they found that subjects who reported use of cigarettes during either or both of the first two assessments were more likely to be missing by the third assessment. A similar pattern was found for users of alcohol and marijuana in one of the studies, but not in the other. Thus, the students who remained in the study were significantly less likely to be users of cigarettes, alcohol, and marijuana than were those who dropped out. At least for these two studies, it appears that the results may not be generalizable to all students.

We were prompted by Hansen et al.'s findings to examine whether the dropouts in our own prevention research differed systematically from those who remained in the study. A Multivariate Analysis of Variance (MANOVA) comparing dropouts and remainers yielded a significant overall effect, confirming that the threats to external validity that Hansen et al. (1983) identified are not unique to their studies. Table 3 presents comparisons of missing and remaining subjects.

Subjects who were missing were significantly more likely to report cigarette smoking, alcohol consumption, and marijuana use at the Time 1 assessment. The parents of dropouts were significantly more likely to be smokers; so too were the peers and siblings. Two questions were designed to assess the subjects' intentions to smoke. On both of these items the dropouts were more likely to indicate that they expected to be smoking at some time in the future. The parents of missing subjects had had less education than those of remaining subjects, and the missing subjects had lower education aspirations. Finally, the missing subjects reported that they received more offers of cigarettes per week than did the remainers.

Table 3
Significant Differences Between Missing and Remaining Subjects
Cigarette and Other Substance (Self-Reported) Use

	6-Month Follow-up					1-Year Follow-up				
	Missing	Remaining	F	Y	Sig	Missing	Remaining	F	Y	Sig
Cigarettes/week	11.45	2.55	68.49	1,1719	0.00	10.51	1.85	78.36	1,1719	0.00
Pot/week	1.19	.396	47.88	1,1711	0.00	1.12	.33	55.80	1,1711	0.00
Drinks/week	1.49	.89	23.07	1,1718	0.00	1.40	.85	24.90	1,1718	0.00
Cigarette Use by Parents, Siblings, and Peers										
Mother smokes	2.57	2.28	14.11	1,1720	0.00	2.57	2.24	21.46	1,1720	0.00
Father smokes	2.87	2.62	18.34	1,1693	0.00	2.91	2.58	22.59	1,1693	0.00
Brother smokes	.45	.35	3.93	1,1703	.047	.48	.32	11.99	1,1703	.001
Sister smokes						.48	.33	11.02	1,1705	.001
Best friend smokes	1.70	1.84	35.61	1,1709	.000	1.69	1.81	70.56	1,1709	.000
Number of 5 closest friends smoke	1.31	.83	38.27	1,1836	.000	1.40	.74	86.71	1,1836	.000
Intentions to Smoke										
When I get older, I will smoke every day	4.39	4.61	12.97	1,1662	.000	4.32	4.67	40.72	1,1662	.00
Possibility of smoking one year from now	1.84	1.55	17.10	1,1683	.000	1.89	1.49	36.41	1,1683	.000
Educational Environment or Aspirations										
Parents' education	Missing	Remaining	F	Y	Sig	Missing	Remaining	F	Y	Sig
Mother	4.15	4.44	26.82	1,1697	0.00	4.18	4.47	30.37	1,1697	.000
Father	4.22	4.44	10.43	1,1655	.000	4.21	4.47	18.52	1,1655	.00
Subject's aspirations	3.14	3.38	18.67	1,1847	.000	3.16	3.40	23.47	1,1697	.00
Offers/week	1.37	.54	38.21	1,1719	.000	1.37	.42	67.68	1,1719	.00

On the basis of these results and those of Hansen et al. (1984), it appears likely that those who drop out of prevention studies are precisely the young people who are most in need of a prevention program. Thus, it is possible that programs that have been shown to be effective in preventing smoking cannot be generalized to more "hard-core" adolescents.

Internal Validity

With regard to internal validity, Hansen et al. (1983) suggest that prevention studies should examine whether there are differences in the rates of attrition among experimental conditions. They found evidence of a difference between conditions in attrition rate for one of the studies that they reexamined, but not in the other. The second issue they raise with respect to internal validity is whether pretest scores for dropouts are different among conditions. They did not find evidence of such a difference in either of the studies they reexamined.

When we examined the data from our smoking prevention study in a similar way, we found no differences between treatment and control conditions in the rate of attrition. When we compared those who dropped out of treatment with those who dropped out of the control condition, we found no differences in the rate of smoking in the prior week, in the number of times they had smoked marijuana in the prior week, or in their consumption of alcohol in the past week. It should also be pointed out that the existence of pretest differences for remainders is suggestive of differential attrition, but does not conclusively prove it. Pretest differences can be a normal outcome of the randomization process.

An alternative analysis strategy suggested by Cook and Campbell (1979) involves testing for condition by attrition interaction effects. A significant interaction would indicate differential attrition across conditions. When we examined our data, we found a significant interaction for pretest smoking rate, $F(1,1719) = 6.40$, $p < .02$, but not on any of the other variables of table 4. Specifically, the pretest smoking rate among treatment group dropouts was higher than it was for dropouts in the control condition, and the reverse was true for those remaining in the study sample. This finding raises doubts as to the internal validity of this and other such studies. That is, if pretest smokers in the treatment group are more likely to drop out than pretest smokers in the control group, then findings showing lower smoking among treatment subjects who remained in the study may in fact be due to differential attrition rather than to the intervention. Clearly, attrition is a threat to the internal validity of evaluations of prevention programs.

Recommendations

Where it has been examined, evidence has consistently been found which indicates that attrition threatens the external validity of smoking prevention studies. Evidence that attrition affects internal validity of prevention studies is more mixed. Perhaps more noteworthy is the lack of analyses

of attrition effects in most studies. Analyses of attrition should be routinely conducted in studies of smoking prevention. The questions posed by Hansen et al. (1983) provide a useful framework.

It would also be valuable to learn more about why subjects are missing from later assessments. One might attempt to determine the extent to which those who are missing a) had transferred to another school, b) had dropped out of school, c) were absent on the day of assessment, or d) skipped the class in which the assessment was being conducted.

Finally, Hansen et al. (1983) suggest procedures to reduce the rate of attrition. These include attempts to trace students who have left the school, and returning to the school to get data from subjects who were absent at the first assessment.

CONCLUSION

Designs in which whole schools are assigned to treatment and control conditions appear to be most appropriate for the evaluation of smoking prevention programs. Researchers and funding agencies will have to come to grips with the fact that such studies will be relatively expensive, yet have relatively poor statistical power. Blocking or matching schools prior to assigning them to conditions will strengthen the power of these studies. Analysis of the data using the individual as the unit is understandable in light of the low power of school-level analyses. However, it must be recognized that use of a unit of analysis that is different from the unit of assignment converts an experimental design into a quasi-experimental design. If one does this, we recommend the use of classroom means as the unit of analysis, rather than individual scores. Studies of the effects of prevention programs that target adolescents who are already smoking would be valuable, since this group has a high risk of continuing to smoke and individuals could be randomly assigned to conditions. In schoolwide prevention studies, the use of passive consent procedures in which parents reply to the investigators only if they are refusing permission is essential, since an active consent procedure produces a biased sample.

Studies of the processes that are presumed to mediate the effects of prevention programs have been heavily biased toward the use of questionnaires to assess cognitive processes. Researchers should clarify what specific events are designated by these cognitive constructs; multimethod assessment could then be used to clarify the degree to which current questionnaire measures actually measure cognitive processes. Since most programs are designed to affect adolescents' social skills and the social contingencies that affect smoking, it

would be informative to develop a multimethod assessment of skills and social contingencies.

Although strides have been made in assessing smoking behavior, we cannot be confident that existing procedures allow us to detect low-rate and experimental smoking. Studies that show that experimental smoking has been deterred may not translate into effects on the prevalence of smoking at the twelfth grade and beyond. For these reasons, it appears imperative that smoking prevention programs be evaluated in terms of their effects on the prevalence of smoking, at least at the end of high school. Separate assessments of program effects on subjects in each level of pretreatment smoking status is needed, and further scrutiny of cessation among regular smokers could be particularly informative.

Subject attrition is a major threat to the external validity of smoking prevention studies, and may be a threat to the internal validity of some studies. Reports of the evaluation of prevention programs should present analyses of the possible impact of attrition on internal and external validity. Studies of the factors that produce attrition could help us to reduce it. Procedures for tracing subjects who are missing from postintervention and follow-up assessments could ameliorate this problem.

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Priorities in Research in Smoking Prevention

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INTRODUCTION

This chapter discusses issues raised by Biglan and Ary in their paper "Methodological Issues in Research on Smoking Prevention". Rather than comment on their paper point by point, I want to take their major themes and develop my own thinking on them.

I use their analysis of units of assignment to suggest that many individual studies of smoking prevention and cessation will inevitably be flawed in multiple ways. Consequently, one should not expect too much from individual studies and should instead rely on reviews for the identification of reliable knowledge, particularly about effective treatments.

I then use their analysis of construct validity to probe the kinds of research questions--and hence the kinds of individual studies--that might have most utility in the study of smoking prevention, suggesting that large-scale planned comparisons of currently advocated prevention strategies might have greatest utility at this time.

Finally, I use their analysis of attrition to probe some of the implications that arise for research design and analysis because smoking seems to be on the decrease in the nation at large. This means that proponents of smoking prevention are forced to evaluate their work relative to a comparison standard that is already moving "by itself" in the very direction prevention researchers want to take it. In this situation, how can we make sure that research on smoking prevention in schools is addressing issues that are likely to add significantly to current trends?

THE UNIT OF ASSIGNMENT AND ANALYSIS

The Problem

Research on smoking prevention in school settings raises the methodological issue of what should be the unit of treatment assignment and of data analysis. As in all school research, the options are many. One can choose to assign school districts, schools, grade levels, classrooms, or individual students. The generally acknowledged principles to follow in unit selection are two.

Principle 1. The unit of assignment should be the same as the unit of analysis. Otherwise the assumption of independent errors is violated, and differences between the higher-order units (e.g., schools) are attributed to differences between the lower-order units (e.g., students). Consider the case where there are two schools in each of two treatment groups and some event other than the treatment influences smoking rates in one of the schools. This theoretically irrelevant event would influence the scores of all or most of the children from the school, entailing a change in the mean of the treatment group to which they were assigned. If we knew that the theoretically irrelevant force in question had indeed operated, then we would know that, in the absence of other analyses, we could not distinguish between whether the irrelevant force or the treatment had caused the observed effect. However, if we did not know about the irrelevant force, we might then conclude that the treatment had caused the change. Yet this would be an error, for the change in the example was due to the treatment-correlated force that influenced only one of the schools but many of the individual students within that school.

In a sense, using students as the unit of analysis provides too many degrees of freedom, since schools were originally assigned to treatments, not students. Under most conditions, inflating the degrees of freedom is likely to cause spuriously low estimates of standard errors and hence produce too many statistically reliable differences. This is a second consequence of the mismatch between units of assignment and analysis, over and above the possibility of systematic bias in treatment effect estimates.

In the hypothetical example above, the technically correct procedure is to calculate a mean score for each of the four schools and to use these means as the raw data in subsequent inferential analyses that contrast the treatment and control groups. However, in our example, this technically correct procedure results in only four observations, two per treatment, making it easy to see one of the major reasons why Principle 1 is often violated. The result is data analyses that involve more units than were originally assigned to treatments, and that therefore increase the chances of biased estimates of treatment effects and of standard errors.

Principle 2. The unit selected for assignment should be the lowest compatible with the integrity of the the treatment contrast. This principle indicates that it is better to assign lower-order than higher-order aggregates--e.g., students rather than classes, intact classes rather than entire grades, and grades rather than whole schools.

One rationale for this is that it is easier to command the full attention of individuals than of classes and of classes than of schools, making it easier to standardize treatments and deliver them more powerfully. A second rationale is that individual assignment permits more comprehensive explanation and prediction because statistical interactions can then be probed at many more levels. That is, data from individual students can be used to examine interactions between treatments and attributes of either individuals, classes or schools, whereas school-level data can only be used to probe interactions between treatments and attributes of schools or school districts, but not between treatments and attributes of either students or classes. A third rationale is that, when the number of higher-order aggregates is very small, statistical power to detect true differences will be low. Assigning lower-order aggregates can dramatically increase the power of statistical tests, as is most obvious when there are few schools but many students in a study.

Although the advantages of assigning lower-order units are clear, it is not always desirable to carry out the treatment assignment at the lowest order of aggregation. One circumstance arguing against the practice is when a treatment is meant to influence classroom or school norms. When individuals are assigned to a smoking prevention treatment that successfully modifies classroom norms, the behavior of control group students within the same class is likely to be affected and the desired treatment contrast will have disappeared. Likewise, if a treatment is meant to influence school norms but is assigned to individuals or classes, a change in the school norms will influence all children, irrespective of their original treatment assignment. As a result, the integrity of planned treatment comparisons will be compromised.

The second circumstance arguing against the principle of assigning lower-order units is when students in one treatment (or control) group may be exposed to a different treatment, perhaps visually or as a result of informal conversation between students during or after school. This situation is not dissimilar from what happens when norms are changed. However, with treatment diffusion students are inadvertently exposed to the instruction in smoking prevention, whereas with norm change students are exposed to the target behaviors of the study without necessarily observing the treatment itself.

For children or classes to be viable as the unit of both treatment assignment and data analysis requires demonstrating (1)

that there has been no dissemination across treatment groups of knowledge of the different treatments; and (2) that students in one treatment group have not been exposed to others' reactions to the treatments they received.

The unit of analysis influences not only the likelihood of bias-inducing social comparison, but also the statistical power to detect true treatment effects. However, the relationship between the unit of analysis and the extent of power loss or gain is complex. On the one hand, the more aggregated a unit of analysis is, the more stable are the measures it provides. This suggests that school means are usually more stable than classroom means and that classroom means are more stable than scores from individual children. The desirability of reliable measurement suggests preferring higher-order over lower-order aggregates as the unit of data analysis.

But arguing against this are three other considerations. First, the more aggregated is the unit of analysis the fewer are the degrees of freedom and the less is the power to detect true differences. Second, the higher the unit of assignment the fewer are the statistical interactions that can be probed. Thus, when entire classes are the unit, interactions involving student characteristics remain unexamined. Third, some children are absent from school more often than others, and some understand the treatment better than others. Consequently, the strength and integrity of a treatment can vary from child to child. This source of extraneous variation will go unanalyzed if the analysis is restricted to higher-order aggregates.

We see then, that the relationship between statistical power and the unit of analysis depends on many factors, some of which will be unique to a particular study. Decisions about the preferred level of aggregation have to depend on a complex interplay between the sample sizes available, the reliability of measures, the integrity of treatments, and the likelihood of detecting statistical interactions involving lower-order units.

Practical Partial Solutions for Research on Smoking Prevention

To stay true to the two principles of unit selection outlined above, one could use schools as the unit of assignment and analysis, but in ways that increase degrees of freedom, minimize irrelevant scores of variability in implementation, and ensure a high level of treatment relevance to all or most of the children in a school. Alternatively, one could use classrooms or individuals as the unit of assignment and analysis, but in ways that minimize the diffusion of treatments or their consequences. As always, though, there is a third step that could be taken. One could try to break the set in which the unit issue is traditionally conceptualized. We now discuss these three possibilities.

Schools as the unit of assignment and analysis. If schools have

to be the unit of assignment, it is desirable to include as many of them as possible in the research design. But how can this be done with a fixed budget in ways that do not compromise the integrity of treatment implementation because of the logistical problems associated with large scale research involving many schools?

One thing to explore is whether it is possible to modify sampling plans to contact only a random subset of children (or classes) within schools and to use the resources saved to contact more schools. The rationale for doing this is that, after a certain point, each additional student lost within a school detracts only trivially from the precision of estimates, whereas when schools are the unit of analysis, a few more schools may lead to a considerable marginal gain in both precision and bias reduction. In some instances, the nature of a treatment or school may require that all students be exposed to a treatment, making the random selection of students impossible when it comes to treatment exposure. But our major point involves the random selection of students for measurement, though financial savings are obviously greater (and more schools can be added) if some students are dropped both from treatment implementation and from the measurement of outcome and process. However accomplished, the strategy of sampling students within schools has a major problem, for many students have to be dropped to save enough money to add just a few schools.

When working with small samples, a second possibility to explore is the assignment of schools to treatments from within strata that are highly correlated with the research outcomes of most importance. This usually means (1) deciding on a blocking variable (say, the percentage of estimated smokers); (2) measuring schools on this variable prior to treatment assignment; (3) creating blocks of schools that differ in the percentage of smokers, with the number of schools in each block being some multiple of the number of treatments; and (4) randomly assigning schools to treatments from within each of the blocks formed by the smoking variable.

This stratified random sampling plan has a number of advantages, listed in terms of descending importance. First, compared to treatment assignment without stratification, it provides greater initial comparability between the various treatment groups, especially when the sample size of schools is tiny. This is because the prior stratification procedure ensures that random assignment only takes place among schools that are initially highly comparable with respect to the major outcome variable. Second, stratification increases the power to detect effects because the variance attributable to the blocking factor can be estimated and removed from effect estimates. And third, if a large sample of schools is available, interactions between the treatment and blocking variables can be estimated.

Although many matching variables are possible, for most studies a

measure of the percentage of pretest smokers is likely to be preferred. This is because it is nearly always the highest correlate of the outcome variable that most substantive experts consider most important: viz, the percentage of smokers at the posttest. However, if some other outcome variable is of greater importance in a particular study, a pretest measure of it will usually provide the best stratification possibility. This is because, over time, measures usually correlate better with themselves than with other variables. If matching on multiple variables is possible, variables should be selected for their ability to add to the total prediction afforded by pretest measures of the major outcome variable.

A third step analysts should take to get around the small sample problem associated with higher-order units is to increase the number of measurement waves. This is because the total number of degrees of freedom is determined by the number of schools multiplied by the number of times measurement occurs. Multiple pretest measurement waves has the additional advantage that it provides a gross handle on patterns of historical shifts in smoking, which is important at present because smoking rates seem already to be on the decline. If more than one pretreatment measurement wave is not possible, it is still desirable to increase the number of measurement waves during protracted treatments, after a treatment has been discontinued or--preferably--at both time points. The more often measurement occurs, the better will be the prediction of time changes within and between treatment groups, making the statistical and visual comparison of time trends more sensitive than would be the case if measurement were restricted to only a single pretest and a single posttest measurement wave.

Although the benefits of multi-wave studies are extensive, they have to be weighed against the potential disadvantages of obtrusiveness and attrition. While steps can be taken to reduce the magnitude of the obtrusiveness problem--largely through less salient measurement, longer time intervals between measurement waves, and complex matrix sampling designs that do not require measurement of each student at each time point--these techniques are not universally applicable. Moreover, while fairly obvious steps can be taken to reduce attrition, these procedures are only tractable within limits. Nonetheless, everything possible should be done to limit the problems of obtrusiveness and attrition, and to measure behavior in as many waves as budgets, hospitality, and funder patience permit.

To increase the likelihood of detecting true effects, analysts can adopt a fourth strategy. They should attempt to replicate administration of the treatment. If evidence of an effect has already been manifest with "experimentalists", one way of achieving this is to have the original "controls" switch and be belatedly exposed to one of the treatments (Cook and Campbell 1979). Designs in which the original controls belatedly adopt an experimental status are common in behaviorist psychology, where

small sample sizes are the norm and causal inference is routinely made to depend on the controlled introduction, removal, and reintroduction of treatments. In this tradition, much less reliance is placed on mechanisms for justifying causal inference that are commonplace elsewhere--viz, a long string of pretreatment measures (as in interrupted time-series work), random assignment (as in classical experimental design), or causal modeling (as in economics or sociology).

A fifth strategy available to prevention researchers is to conduct their work in schools where the kinds of student are particularly likely to begin smoking or to become heavy smokers. At present, most prevention research is directed at entire schools. But many of the children in these schools will never be smokers, or will never be smokers at a level currently considered to be health threatening. Consequently, many (if not most) of the children cannot be helped in the most important ways that provide the political and economic justification for smoking prevention programs. Moreover, the power to discriminate between treatments is greater when schools are selected for study because they contain large percentages of the types of children that descriptive research has identified as particularly likely to go on to smoke. At least this is the case if one can assume that the efficacy of a smoking prevention treatment does not depend on the level of initial smoking in the school. If the potency of treatments is lower in schools with more smokers, then two countervailing forces are set up that influence the power to detect effects. On the one hand schools with more smokers will have more students who could change; but on the other hand, the environment in these schools is less conducive to bringing about individual facts.

Other than in a trivial physical sense, schools are not fixed entities and there are daily and annual fluctuations in the children who attend them. A sixth way to increase the chances of detecting true effects is to limit the data analysis to purified experimental and control samples that contain only those children who have provided data at all the measurement waves. The data from children who were absent on any day of data collection should not be used, including the data from those who enrolled after a treatment. Such a procedure refines the unit studied. It is no longer the school; it is now the school reconstituted as those children who provided data at all measurement waves. (Of course, if one is worried lest those who drop out of school are the most susceptible to smoking, it is theoretically possible to track them down and keep them in the measurement plan and data analysis. Tracking them is an expensive process. If implemented, as much effort should go into contacting controls who drop out as experimentals. Indeed, the general principle holds that experimentals and controls should be treated identically in all respects other than treatment assignment).

Finally, when schools are the unit of analysis careful monitoring is required over the total course of a study. This is primarily

to check on the fidelity of treatment implementation and to detect any irrelevant school-specific events that might have affected the major dependent variables. For instance, some classes within a school may not be exposed to the treatment for irrelevant reasons; or a local community or hospital might begin its own health promotion program that influences some of the students in a school and threatens to distort the treatment effect estimates. Monitoring for such events is required and can be accomplished in many ways that differ in cost and accuracy (Cook, Leviton, and Shadish 1985).

To summarize: Smoking prevention research that requires higher-order aggregates runs the risk of bias and imprecision because of the small sample size of units. To reduce the problems I suggest: (1) sampling students within schools rather than taking all students, and using the resources saved to add more schools; (2) stratifying schools on the major outcome variable prior to random assignment to treatments from within strata; (3) adding waves to the measurement plan so as to increase the degrees of freedom; (4) implementing a switching replication design so that the original control group receives belated exposure to one of the experimental treatments; (5) selecting schools for study because of their known high levels of smoking; (6) purifying the data so as to examine in some analyses only those students (and their yoked controls!) who have had exposure to the treatment and have been measured at each testing wave; and (7) careful monitoring to assess treatment implementation and to observe whether any unexpected school-specific events have occurred that might be correlated with the outcome variables of greatest importance.

Classrooms as the unit of assignment and analysis. Classes are only viable as the unit of assignment if it is reasonable to assume that there will be minimal seepage of the effect or of the treatment across classes. Estimates of the likelihood of such seepage depend on the particular nature of a treatment or effect, and I am not going to speculate in detail about the treatments and effects that do and do not facilitate seepage. However, it should be noted that some classroom-level studies have been successfully conducted in the health education field after pilot work was carried out and showed that treatment diffusion was not likely (e.g., Connell and Turner, in press). The issue we now address is when one can use classrooms as the unit of assignment without having to aggregate up to whole schools and absorb the limitations that go with schools as the unit of assignment and analysis?

With some types of treatment or effects, I assume seepage is more likely across classes within a single grade than it is across different grades. The rationale for this is that students are more likely to communicate with persons in their own age cohort than with younger or older students. If true in particular instances, this assumption implies that one can randomly assign grade levels to treatments within schools, with each class being

assigned to the particular treatment its grade level receives.

If adjacent grades are likely to communicate with each other despite the one year age differential, then immediately adjacent grades can be skipped. One might, for instance, assign Grade 9 in High School A to the experimental treatment, with Grade 11 becoming the controls. Then, in a matched high school, the reverse assignment would take place so that Grade 9 become the controls and Grade 11 the experimentals. This yoked assignment procedure is then continued until all the high schools in the sample have been allocated, resulting in a sample size that is twice what it would have been if schools were the unit of assignment. Also, the variability between schools can be removed from the error term, and the measures are likely to be particularly reliable since they are based on all the children and classes in a particular grade. However, strictly speaking, classes are not the unit of assignment in the above case. Grade levels are.

There are some treatments that fit easily into existing range of expected school curricula and are not likely to stand out. They are seen by students as being just more instruction, and are not likely to be talked about. Under these conditions of "mundane realism," it is possible to deal with smoking in some ways in some classes while dealing with it differently or not at all in other classes at the same grade level. In such a scheme, the class assignment to treatments is at random, occurs between classes within grades, and is replicated across all the schools in the sample under study.

Since the crucial assumption is that little seepage will occur, it is important to ask when treatments are likely to be so much a part of expectations that they are rarely (if ever) talked about outside of the classroom. The assumption strikes me as more reasonable when different versions of an antismoking treatment are being contrasted than when one version is contrasted with no such instruction at all. Nonetheless, Connell and Turner (in press) were able to assume mundane realism even in this second context because students knew there were so few health education teachers that not all classes could receive instruction in the same quarter. Turn-taking was the norm. Although treatments are very salient to researchers and curriculum developers, they should not assume that the treatments they hold dear are as salient to students as they are to themselves. Ultimately, the issue of treatment seepage is empirical and deserves the considerable effort being expended on it during pilot research.

It is important to note that inferences about treatment effects can be biased, not only because of the seepage of treatments, but also because of the seepage of effects. Consequently, within-school comparisons do not make sense (1) if one is trying to influence school-wide norms; or (2) if one's substantive theory supposes that peers from different classes within the same school are a potent cause of other students taking up smoking.

In each these cases, treatments that are effective in reducing smoking will also reduce smoking in "control" classes that have never been exposed to the treatment being evaluated. Underestimates of a true effect will then result.

If the assumption is made that seepage of either a treatment or a dependent variable is likely to be widespread within a school, then assignment and analysis need to be between schools rather than between classes or grades. It will not be possible to assign individual students. For students to be viable as units requires no seepage and either "pull-out" prevention programs like those found in bilingual and remedial education or programs that are exclusively based on giving different children different homework assignments. Such individual-level programs would not only be obtrusive and expensive; they might also be of such a restricted range that only weak treatments would be possible.

Breaking the set in which the unit problem is conceptualized. I have tried to show above that there is no single unit of assignment or analysis that is "correct" or "best". The preferred choice depends on many factors, including: The nature of the treatment and its presumed diffusability, the configuration of classrooms, the likelihood of influencing school norms, tradeoffs between the sample size of schools, classes, and students, and the assumptions one makes about the likelihood of different forms of statistical interaction. While no easy answers are possible about the selection of units, the two principles I have outlined from classical theory should nonetheless be important inputs into the process of deciding what level of assignment and analysis to choose.

In school and community studies where the nature of the treatment dictates studying a few, large aggregates, one way to break out of the traditional set in thinking about unit selection is to assume that Principle 1 can be violated. Asymmetry between the units of assignment and analysis is least serious in its negative consequences when the sample of higher-order aggregates (e.g., communities or schools) is "large" and extensive on-site monitoring has shown that no outcome-related idiosyncracies occurred during the study to affect more of the units from one treatment group than another. Under these conditions, one might indeed assign communities or schools to treatments but analyze the data as though they came from individuals.

While asymmetry between the units of assignment and analysis is currently widespread in prevention research, it rarely occurs under the favorable conditions outlined above. Indeed, my judgment is that such asymmetry is most likely to occur precisely because the small number of communities or schools precludes statistically powerful analysis. But even if hundreds of children change their behavior when few schools are assigned to each treatment, the possibility always exists that these changes are the result of a single theoretically irrelevant force unique to one or two of the sites--e.g., a local smoking drive, a

particular principal who forbids teachers to smoke on the premises, or a local celebrity who dies of emphysema, etc. Although necessity is a not irrelevant justification for the asymmetry of units of assignment and analysis, it seems to me that nearly all individual research reports would profit by providing more commentary about the gains and losses that follow from having violated Principle 1.

A second way to break out of the traditional set implied by Principles 1 and 2 is to conclude that no single study with a small sample of schools is going to provide convincing inferences about the effectiveness and transferability of prevention treatments. Instead, progress is to be expected from studies using methods of quantitative research synthesis, among which the most visible today is metaanalysis. I suspect that dependable knowledge of the ongoing multi-risk, communitywide prevention studies being conducted at Stanford, Minnesota, and Rhode Island will come, not from the individual trials now underway with their small samples of communities, but from combining the studies. Such a synthesis would use community means as the basic unit of analysis to create for each study a set of pretest/posttest means per treatment that will be used as the basic data for inferential analyses. In school research, metaanalyses would use school or class means as the basic input.

The advantages and disadvantages of metaanalysis have been outlined many times elsewhere (e.g., Cook and Leviton 1980), and I will not go into them here. But if a sufficient sample size of smoking prevention studies exists, the feasibility of a metaanalysis might be worth exploring since the units of assignment and analysis would not clash. One would therefore escape from the conundrum associated with most individual studies: Either a small number of schools is assigned to treatments and the resulting data are analyzed correctly, but with little statistical power and some likelihood of bias from "local history" (Cook and Campbell 1979); or else the data are analyzed incorrectly in ways that provide more degrees of freedom but that also bias estimates of both standard errors and treatment effects.

This is not to suggest that I am against all individual level analyses. They are absolutely required to explore interactions between treatment and student characteristics. It is merely to suggest that such exploratory analyses have to be done together with analyses that use the appropriate units of analysis. These are the units of original treatment assignment.

CONSTRUCT VALIDITY AND VARIETIES OF CAUSAL STATEMENT

Biglan and Ary point out that most of the interventions tried to date are more like multivariate treatment packages than unidimensional constructs with an obvious theoretical referent; that prevention research requires important but difficult judgements be made about how far along hypothetical chains of

causal influence (Flay and Cook 1978) one should go before declaring that a treatment is "successful"; and that it would be useful if prevention studies could specify the causal processes that occur between treatments being implemented and impacts being observed.

These are all sensible points, and they speak to a crucial strategic issue. If one follows through on the implications of Biglan and Ary one would multiply measure each individual component of each of the treatment constructs invoked in each of the many causal models that could easily be developed from all the current substantive theories of relevance to smoking prevention. Then, one would analyze the data using some form of causal modeling, perhaps LISREL. While I generally support such a plan, I would like to point out its side effects, particularly with respect to expense, the obtrusiveness of measurement, and the opportunity costs of sinking so much into the fine-grained measurement of process.

In basic research we seek to identify causal explanations for phenomena, hoping to achieve simple results. Initially, we ask questions like: "How do humans grow physically?" We learn to answer: "Because our bodies contain DNA and RNA that genetically control the messages sent to cells through a process in which...." We also ask: "Why do we see?" And we learn to answer: "Because our eyes are constructed in such and such a way and are connected to the brain in such and such a way so that when light falls on the retina, such and such happens." We learn to ask: "Why does flicking the lightswitch cause the room to illuminate?" An electrician might respond: "Because a house is wired in such and such a way; its wiring is connected to an external source of electricity that enters the house; and so on." In all of these cases we equate causal explanation with knowledge of micromediating causal pathways that bring about some important result; and these pathways are typically specified at a level which seems more reductionistic than the phenomena they are being used to explain. Nobel Prizes are awarded for explanations of this type, and scientists are taught to seek such explanation as the primary goal of their work.

Contrast such knowledge of causal explanations with the following examples of knowledge of descriptive causal relationships: Rubbing two sticks causes fire; flicking the light switch causes the room to illuminate; taking aspirin causes headaches to go away; and taking chlorpromazine causes the symptoms of schizophrenia to disappear. These examples all illustrate dependable knowledge of causal relationships, and we can use this knowledge to light fires, get rid of the dark, alleviate headaches, and obviate the socially uncomfortable manifestations of schizophrenia. Yet few of the lay persons who know of these causal relationships can explain them. For explanation we turn to scientists and relevant professionals. Unfortunately, scientists and professionals cannot fully explain all of the causal relationships above. I am told we still do not know why

aspirin or chlorpromazine are effective. They were discovered as side effects in investigations of other issues; they are not the product of deductions from basic knowledge of explanatory processes.

Moreover, it seems reasonable to assume that homo sapiens had dependable, useful knowledge of the consequences of rubbing two dry sticks together long before anybody could explain why the process of rubbing sticks results in sparks that can set tinder alight. Causal explanations of micromediation are not required for knowledge of useful and dependable causal connections. Indeed, utility can easily be demonstrated to be independent of explanation. For instance, we not only do not know why chlorpromazine works; we know that it fails to cure and only suppresses symptoms. Yet symptom suppression is often socially useful. We also know that chlorpromazine works better with some schizophrenics than others. Its efficacy is probabilistic, not deterministic. Even so, the drug is efficacious often enough to be socially useful.

I do not want to argue against the utility of causal explanations, for they often suggest totally novel causal relationships that lead to the development of causal manipulanda of practical value. Indeed, the major economic justification for basic science has always been that the explanatory knowledge it generates helps identify practical and useful causal agents. Actually, causal explanatory knowledge does more than this. It also implies knowledge of the contingencies on which more molar causal relationships depend, helping predict when a causal agent will and will not be effective. Thus, once one knows the processes whereby rubbing sticks causes sparks, one can then infer that rubbing wet sticks together will not result in sparks unless one has rubbed so hard and for so long that the sticks have dried out.

Despite these very real advantages, knowledge of micromediating processes is not necessary for knowledge of contingent causal relationships. Knowledge that dry sticks cause sparks, but wet ones do not, can be acquired through practical problem solving designed to understand why rubbing sticks has sometimes caused sparks in the past but at other times has not. A curious ancestor of ours many generations ago might well have deliberately experimented with various kinds of stick, various ways of rubbing, and various forms of tinder in a number of different social contexts before discovering through trial-and-error that only dry sticks lead to sparks. While knowledge of contingent causal connections can follow from the complete causal explanation of micromediating processes, such explanation is not required for identifying either causal agents or the contingencies on which their efficacy depends.

In prevention research, the causal agents can be theoretical constructs, individual components of a global treatment package, or individual treatment packages in their entirety. My working

assumption is that in most prevention research the program activities actually implemented are imperfect realizations of imperfect substantive theories. It follows from this that implemented treatments are influenced by many factors not in the relevant substantive theories. Indeed, it is well-nigh inevitable that practitioners will bring their own discretion to bear to fill in theoretical gaps and to mold treatments to what their own experience has taught them are the unique and important attributes of the particular schools, children, and teachers with whom they are associated. In the world of action, treatments reflect many origins and are inevitably multicomponential. They are not the presumed unidimensional causal forces of most basic research, although they may include such components as part of the total treatment package.

The above discussion implies four types of causal knowledge at play in ameliorative social research:

- (1) knowledge of the effects of substantive theoretical forces that are embodied as part of a total program design and are believed to be unidimensional;
- (2) knowledge of the effects of a global, multivariate treatment package that includes many components not specified in formal substantive theory;
- (3) knowledge of contingent causal relationships that specify the conditions under which a given global treatment differs in effectiveness; and
- (4) explanatory causal knowledge that specifies both the causally efficacious components of the treatment and the particular micromediating processes through which influence is transmitted to the causally impacted components of the dependent variable.

Although these four types of knowledge are related to each other in multiple, complex ways, they are heuristically distinct. In the context of smoking prevention programs for school settings, knowledge of theoretical antecedents is equivalent to learning about the consequences of, say, inoculation against peer pressure to smoke. Knowledge of causal relationships is equivalent to learning about the effects of a global program--e.g., Smith's program to prevent smoking. Knowledge of contingent causal relationships is equivalent to knowing about the conditions under which Smith's program has the effects it sometimes has. Finally, knowledge of causal explanations is equivalent to specifying the causal pathways through which Smith's program has its effects--e.g., the program is effective because, on the one hand, it promotes an inoculation that leads to resisting peer pressure to smoke and because, on the other hand, the program also increases the social support parents provide to their children. This illustrates only two causal pathways to decreased smoking, and very simple ones at that. Many more paths are possible, as are many more necessary events on each path.

These distinctions between types of causal construct raise

consciousness about what should be the priority form of causal knowledge in prevention research. In much basic research in the social sciences the hope is to manipulate or measure unidimensional causal agents derived from substantive theory about, say, dissonance or communicator credibility. At issue are tests of the antecedents and consequences of presumed novel entities, and in this tradition construct validity is equated with the construction of research operations that fully reflect formal theoretical definitions. However, in more applied research the abstract concepts to which one wants to generalize are less precise and might be more like a methadone maintenance or Headstart center than like the theoretical entities of dissonance or communicator credibility. While the concepts of a methadone maintenance or a Headstart center may include some apparently unidimensional forces abstracted from substantive theory, they typically include many other components that may or may not be necessary or sufficient for bringing about effects. And their construct validity does not depend on a fit with substantive theory. Instead, it depends on the extent to which a plan for a program is implemented in practice.

One problem with studying treatment packages like Smith's program or a Headstart center is that, by themselves, they rarely permit identification of the conditions under which a treatment is effective. Yet such knowledge is vital if one assumes a social reality where the same apparent treatment may be implemented differently in different settings or where reactions to the same treatment may be different with different types of people. Knowledge of such contingencies is believed to be crucial, not only among scholars, but also among planners who want to be able to plan more exactly and among practitioners who want to tailor their activities to important sources of variability in clients and settings. Thus, knowledge of causal contingencies enhances prediction, explanation and control.

Even so, it does not offer total prediction, explanation or control. These require a full understanding of all the processes that occur after a cause has varied and before an effect has been measured. Only with such knowledge can one identify all the necessary, and some of the sufficient, conditions for bringing about an effect (Cook and Campbell, in press).

Since the different conceptions of cause promise different types of knowledge and different degrees of prediction and control, we have to ask: Which is to be preferred in prevention research at this time. Various groups with a stake in social programs assign different utilities to these types of causation (Cook 1984). Scholars who work in university settings have been trained to appreciate the development and testing of substantive theory, and are inclined to assign a higher priority to theorytesting than to the study of multivariate treatment packages. Since they are also trained to appreciate the advantages of explanatory theory, they also tend to place a higher value on identifying molecular causal explanations than on describing more molar causal

relationships. This last preference leads them towards the study of causal contingency and especially towards the identification of micromediation processes. Few basic researchers value learning about the effects of a treatment package that is so global (1) it cannot be fully described in theoretical terms, (2) its consequences are produced only intermittently, and (3) its causal mechanisms are not understood.

Many high-level Federal administrators and Congressional officials have a different set of priorities, particularly if they work for action agencies. In addition to wanting to keep their jobs and protect their agency budgets, they also want to see some amelioration of the acknowledged social problems for which their agency or office is held accountable. At the same time, most of them appreciate the long time lines that may be required to develop, test, and modify substantive theories and to ferret out the most important contingencies and causal pathways.

They also know that, in designing programs for widespread implementation, logistical, political, and economic realities restrict what they can promote.

In particular, they often cannot use fine-grained knowledge. For instance, while it might be useful to target an intervention that works only with middle-class children at predominantly middle-class schools, administrators know that the intervention will often have to be made available to all schools, or at least to all those that want it. Despite the political pressures towards universal distribution, there are nonetheless some occasions when Federal or state resources are targeted at poorer schools. But when this happens, local superintendents and school boards often anticipate that parents will object to more being done for poorer children. In the past their anticipation has sometimes led them to provide discretionary resources they control to the more affluent schools in their districts. While this helps them avoid the political repercussions stemming from Federal funds that are to be unequally distributed, it can obliterate planned "treatment" contrasts.

The moral is that the fine-grained, contingent knowledge to which science aspires cannot always be applied with the same degree of fineness in the more coarse-grained world of social action. Educators are not like engineers. They cannot take theory-derived, deterministic processes and neatly pack them into closed systems, as occurs with car engines placed under hoods or heating units placed in houses. In the open system world of social policy, statistical main effects have more currency for practical action than statistical interactions. Indeed, certain interactions have to be treated as though they do not exist even when it is known they do. Whether deliberate or inadvertent, the overlooking of statistical interactions usually has trivial consequences if the interactions are of the form in which all groups or regions benefit from a treatment, but some more than others. The consequences of overlooked interactions can also be

tolerated in many instances when some groups benefit from a treatment, but others do not even though they are not harmed by it.

The major problem arises when policy makers are forced to implement course-grained changes but the more fine-grained social reality is characterized by some groups gaining from a treatment while others are actually hurt by it. Our speculation at this time is that, in prevention research, statistical interactions in which some groups gain and others lose are much less common than interactions in which (1) all groups gain, but differ in the amount of gain; or (2) some groups gain, but other groups neither gain nor lose. In any event, both the mandate of government officials and the political realities in which they operate argue for the primacy of knowledge about what dependably works, even when one is totally or partially ignorant about the construct validity of the causal agent, about the conditions under which the agent is effective, or about the processes that make the agent effective.

Cronbach (1982) has sought to bridge the gap between academic and government priorities about cause, arguing (1) that policymakers want knowledge of transferrable programs that can be implemented from site to site across the very heterogeneous USA; (2) that knowledge about transfer can best be inferred from knowledge about causally efficacious theoretical constructs, contingent causal relationships, and explanatory micromediating processes; but (3) since policymakers do not require the same level of uncertainty reduction as academics, the time- and labor-intensive methods that are traditionally used in science to assess knowledge of causal constructs, causal contingency, and causal understanding are not required to meet the information needs of policymakers and practitioners. Cronbach aspires to a "reasonable handle" on theoretical knowledge rather than the complete uncertainty reduction that motivates scientists. With this knowledge he hopes to achieve the educational equivalent of going to a house without lights, "roughly" knowing how to run electricity into it, how to set up the wires, and how to construct switches and bulbs. With this knowledge he can then make light.

While Cronbach's general thesis is useful in identifying the primacy of learning about transferrable knowledge, it falls in three crucial particulars. First, causal explanation is not the only means of learning what is transferrable, even if it is the most reliable of all the means available. We can also learn what is transferrable by empirical demonstrations that a particular cause-effect relationship has been repeatedly found in the past, either in the particular type of setting to which we desire transfer or across a wide variety of different times, settings, respondent populations, and realizations of the treatment and outcome measures. In this context, I suspect that the popularity of metaanalysis is because it has led to identifying many stubbornly dependable causal relationships that have been

replicated across a heterogeneous collection of studies and so may be transferrable to novel sites and respondent populations. Certainly, the appeal of metaanalysis cannot be based on providing knowledge about causal contingency or micromediation, for few metaanalyses have resulted in claims about such knowledge. And of those that have, few are convincing. The predominant finding in metaanalysis is of a coarse-grained, but demonstrably dependable, main effect of some treatment.

Cronbach's answer also fails to specify the degree of causal explanation that is required for transfer to be reliable enough to be useful. While we used the physical example of light to make points about causation, the reality is that few theories in the social sciences have been successful in coming up with explanatory knowledge that is as valid and detailed as what we know about electricity. Most social theories are more primitive and fail to deal well with the vast contextualism in human affairs, with the reality that important outcomes operate in open systems and are nearly always multiply determined, and with the fact that the relevant theories are very young. As a result, it would be unrealistic to expect social science theories to be as elegant, parsimonious and predictive as theories in the natural sciences. If this is true even with theories tested using the best social science methods, think what confidence we would have in the results generated using methods of lesser stringency than the best current methods of the social sciences!

Finally, it is possible to take issue with Cronbach's assertion that professional practice and federal and state policy require a lower level of uncertainty reduction than formal scholarship. Although our knowledge of the issue is scant, some case study information suggests that policymakers will fail to use knowledge claims that are credibly contested (e.g., Boeckmann, 1976). The assumption is they look to science for knowledge that is beyond dispute and can play "definitive" roles in policy deliberations. While these expectations may be unrealistic, they do not suggest an existing willingness to settle for lower standards of inference.

The implication of this critique of Cronbach is that the utility of knowing about reliable, but ultimately unexplained, causal connections may provide enough of a pragmatic justification for orienting social research towards identifying such connections. This does not preclude adding to the measurement, sampling, and analysis frameworks of a study to probe the theoretical forces responsible for treatment efficacy. But it does suggest that such additions should not consume so many financial and psychic resources that they detract from answers about the implementation and effects of global treatment packages.

The degree of justification for claims about the consequences of such packages increases as a function of the quality of research design, the frequency with which the same results have been obtained in the past, and the degree of heterogeneity in the

times, settings, and populations across which successful replications have taken place. Replications that vary in time nearly always require synthesizing many independently conducted studies. However, desirability in settings and populations can be achieved without studies staggered in time, implying the viability of some form of large scale, multisite study.

Yet such studies have acquired a bad reputation in the past because a large sample of sites can lead to logistical problems in data collection, to low quality treatment implementation, to poorly implemented random assignment, and to inadequate knowledge of what has actually gone on in schools and of what measures actually represent. These problems are exacerbated if several different treatment packages are explicitly contrasted. The "horse race" that results can arouse political tensions, especially since the advocates of some treatments are likely to believe that the dependent variables by which evaluation is made are more relevant to some treatments than others.

My own belief is that the criticisms above were more valid for the first generation of large-scale experiments conducted until about the middle 1970s. The lessons that were learned from them are now being widely disseminated (see Cook et al. 1985 and Cook, *in press*), and the present generation of ongoing large-scale experiments is much more sophisticated (e.g., Connell and Turner, *in press*; Young 1984). Many of the objections to large-scale, multisite experiments that contrast several models now seem irrelevant, and we have begun the move from the era of horse races to an era of yacht races. Yacht races decrease the level of counterproductive competition because many winners are possible, each by different criteria. Since prevention projects are typically evaluated by multiple criteria anyway, it is not difficult to imagine some projects doing better than others in some respects while other projects do better in other respects.

That is not to say that all the past objections to large-scale studies are irrelevant. Three still seem potent. First is the danger of premature experimentation using models that have not been adequately pilot tested. Unfortunately, we have no way of knowing what degree of treatment maturity is sufficient to warrant a serious "clinical trial." The second danger stems from Federal officials needing premature feedback, so that not enough time is available for planning a study, getting all the stakeholders committed to its goals and methods, and getting the results fully probed and critiqued before they are released. The third danger follows from different stakeholders having different information needs. Some might especially want to learn about the treatment and its component parts as theoretical entities; others might want to learn how well a treatment is being implemented; others might want to know what immediate effects the treatment has on one set of indicators; others might want to learn about a quite different set of measures, or a quite different time period of demonstrated effectiveness. Many types of questions about intended and unintended influences should be asked in any single

evaluation, and there is rarely justification for focussing on only one type.

Nonetheless, priority orderings have to be made and have to be specified so clearly that a high likelihood exists of meeting those research objectives considered most crucial. Unfortunately, no algorithm exists for arriving at such priorities for a single study, even though it is obvious that pursuing one research objective as central can take resources away from examining other objectives and can actually lower the quality of answers to some questions. The nature of the trade-off between question types has been actively debated in the evaluation literature (c.f. Cronbach; 1982; Cook and Shadish, in press; Guba and Lincoln 1981), and it is clear that multiple issues should be probed in any one study--but with an exquisite sense of priorities.

I have tried to argue that a compelling rationale exists for learning how well a global treatment has been implemented and for identifying the effects it has had across a heterogeneous array of settings and student populations. In the absence of a large number of studies to be synthesized, such a rationale calls for multisite studies, preferably those that have learned the mistakes of past studies of this type. A lesser emphasis should be placed on identifying the effective components of the treatment and on identifying the conditions under which the treatment or its parts are effective. And least emphasis should probably be placed on identifying micromediation processes. However, clues about micromediation should be forthcoming from probes of the implementation data, from the prior literature and professional wisdom, and from whatever analyses have been conducted to identify both the causally efficacious components of treatments and the conditions under which treatments and their component parts seem to be most effective.

ATTRITION

According to Biglan and Ary, the students most likely to be heavy smokers (and so presumably at greatest risk of heart and lung disorders) are those least likely to stay in a study, irrespective of the treatment to which they were originally assigned. The presumption I make is that this higher overall attrition rate is due to heavier smokers being more prone to drop out of school or to attend class less regularly. I would be surprised if it were due to a greater likelihood of refusing to be in a study during days when the student was actually in school.

The attrition process Biglan and Ary discuss constitutes a serious threat to external validity, for it means that treatments are tested less well with the very high school populations most likely to begin smoking. Thus, the treatments are tested on those who need them least. One consequence of this is

uncertainty about whether the treatments so developed are as effective with the groups that need them most as they are with others. A more disquieting consequence, advanced by McKnight (1982), is that the treatments so developed will be totally ineffective with the populations at greatest risk because they or their representatives may refuse to cooperate with treatments that are inadvertently impregnated with a set of cultural assumptions they find unacceptable.

Biglan and Ary also present data about a quite different form of attrition. They suggest that students who experience smoking prevention treatments are more likely to drop out of a study when compared to the students in no-treatment control groups. At issue here is treatment-correlated attrition rather than the overall attrition discussed above which is not a causal consequence of smoking prevention treatments. If we assume that the Biglan and Ary finding is not due to chance, it is difficult to interpret. It does not seem likely that a simple smoking prevention program would increase the rate at which students stay away from school or leave school for good. A more likely possibility is that the prevention treatment caused more students to drop out of the measurement framework. Even so, it is unclear why students would refuse measurement because they had earlier experienced a treatment. Hence, it is important to examine whether treatment-correlated attrition happens in future smoking prevention studies. If it does, one of its technical consequences will be quite troublesome, for researchers will have to analyze data as though they came from a quasi-experiment rather than a randomized experiment. Data analysis is much more problematic with quasi-experiments, and only with interrupted time-series studies is there anything even approaching consensus about analysis (Cook and Campbell 1979).

Another issue deserves attention when considering sample attrition. This is the practical problem of following up high school dropouts to collect long term data. For nearly all researchers, the poor constitute one of the most difficult to reach social groups in this nation. This is especially true of young, inner-city males who tend to drop out of school early and take to the streets in rates that exceed those of other adolescents and other poor people. Yet the stakes involved in tracing such youths are particularly high for prevention researchers, since they are among the groups with the highest risk of the heart and lung problems that smoking causes. Yet keeping these youths in the treatment and measurement framework of a study creates logistical and financial headaches. It also leads to technical trade offs resulting from the youths' only partial exposure to experimental materials.

The issue of dropouts being particularly in need of the health outcomes smoking prevention is trying to attain relates to a secular trend that is now taking place. For reasons of which we are not yet fully aware, prevention is part of the current Zeitgeist in many health related areas, and the probability of

smoking is already declining among high school students. Thus, the escalator is already going in the direction prevention researchers want to take it. In light of this, we need to ask: What should the objectives of prevention research be if larger social forces are already leading to a decrease in smoking?

A number of possibilities come to mind. Prevention research might continue with its current priorities and the school populations it currently serves in the hope of accelerating the downward escalator. If it is successful in this, many lives and dollars will be saved, the exact magnitudes of which cannot be estimated.

However, a more important result would be if prevention research not only speeded the decrease but also lowered the baseline eventually reached. This would entail a lower level of eventual smoking and medical complications than would have arisen from the Zeitgeist alone. But to achieve a lower base rate entails persuading some young persons not to smoke who would otherwise have done so despite the Zeitgeist.

Who are these persons? While we cannot identify them all, one subgroup stands out: The "hard-to-reach" youngsters who now leave school early or attend only sporadically from ninth grade on. They smoke more than most other demographic groups, and they suffer more often than others from the physical consequences of smoking. Reaching and changing such persons will almost certainly lower the baseline compared to what it might have been, also decreasing the personal misery and costs to society associated with the poorer physical condition of these young people.

Nearly all smoking prevention efforts have been developed by middle-class white intellectuals and mostly tested in schools with a similar profile. It is not clear how well the programs and research experience so developed transfer to schools in poorer neighborhoods where smoking may be embedded in unique cultural contexts and may be uniquely related to the use of other drugs. Will the programs be implemented as widely in these schools as in middle-class ones? Will they be implemented as well? Will they be as effective if implemented as well? These are serious questions to which we do not now have an answer.

One precondition for probing these issues is that prevention researchers include in their studies populations of poor, urban adolescents. A second precondition is that they tackle, head-on, the problems of maintaining contact with these youths after they have left school. While the difficulties associated with each of these tasks are legion, they are not insurmountable and have to be broached if we are to (1) demonstrate long-term abstinence from smoking in these young people, despite all the social pressures in their social environment that impel them to it; and (2) probe whether the final smoking rate stabilizes at a lower rate than would have been the case if the Zeitgeist has done its

work with only the current level of school-based smoking prevention.

CONCLUSION

Things can be done to improve the design of individual studies that have schools as the unit of analysis, but where few schools are available for study. Some of them are outlined in the first section of this paper. However, the conclusion is reached that, despite such improvements in research design and analysis--which we would like to see institutionalized--confident inference about reliable causal connections involving smoking treatments will depend on reviews of multiple studies rather than on single studies. Such reviews are called for on the most important substantive issues in the field.

Four different types of causal knowledge can be gained from field research on smoking prevention and cessation. In the second section of this paper I argue that the highest-priority causal information is needed about "what works" in a global sense. The next highest priority is to begin identifying what it is about global treatments that makes them effective, including analyses of the conditions on which effectiveness is contingent. I suggest that the lowest level of priority should be accorded to knowledge about causal explanatory processes--defined as the identification of micromediating causal mechanisms. Scholars will undoubtedly object to these priorities; and we can be sure that, even if research was funded to meet the priorities designated above, researchers' preferences would nevertheless lead them to expend considerable effort to define treatments better, to identify causal contingencies, and to understand causal micromediating processes. This would not necessarily be counterproductive, for if some knowledge of these types of causation can be obtained without compromising knowledge of the causal connections associated with ameliorative treatment packages, it would be useful indeed.

The priority ordering I prefer leads to the advocacy of large-scale, multisite studies, preferably contrasting maximally different global smoking prevention treatments, and including a heterogeneous collection of schools, implementation measures, and outcome variables. Such studies do not totally conflict with modest expectations about the interpretability of individual studies, since such studies would have multiple schools in each treatment condition and would therefore avoid at least the problems associated with few schools per treatment. Moreover, the studies could be designed to avoid many of the problems recently identified in past multisite studies. And they need not involve a single research contractor carrying out the research. Both Cronbach (1982) and Campbell (1984) have suggested setting up several smaller studies of an issue rather than relying on a single large experiment. The key is multiple replication, whether through metaanalysis, primary data collection done in one large study, or primary data collection conducted simultaneously in

several smaller studies of what appears to be the same issue.

It seems that the high school students most likely to smoke heavily are those least likely to remain in prevention studies. This is serious prima facie. It is doubly serious if one acknowledges that per capita smoking is going down among young people in general. Given this decrease, how can more prevention efforts be justified? One justification is that such efforts will speed up the decrease that is already taking place; a second justification is that such efforts may lead to a lower eventual base rate of hard-core smokers; and a third justification is that prevention efforts are most likely to lower this base rate if prevention efforts are targeted at the urban poor who, as a group, currently suffer most from smoking. The second and third justifications are related. If prevention efforts are not targeted at the heaviest smoking groups that are also least likely to stay in school, it will be all the more difficult to use school-based programs to lower the national base of smokers in general and of heavy smokers in particular.

Work is therefore needed to examine--and, if necessary, develop--treatments that are appropriate for poorer youths. We will also need to ask whether schools might be the most appropriate setting for such studies. If they are--even as only part of the total armamentarium of efforts--the current set of smoking prevention researchers may need to be expanded, or may need new skills, in order to be able to operate in the special ecology of schools in poor neighborhoods.

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Future Directions in Drug Abuse Prevention Research

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Preventive interventions exist today that were unknown a decade ago. These interventions go beyond simple demonstration of the health consequences of smoking and alcohol and drug abuse to approaches theoretically based in human developmental and behavioral psychology. A number of researchers have become interested in testing prevention approaches to drug abuse behavior, and rapid changes have resulted, not only in intervention content, but also in research design and methods of analysis. These changes have provided many answers, as well as new and challenging areas for future research.

The preceding chapters have reviewed research related to three approaches to smoking prevention: social inoculation, social skills, and cognitive-developmental. The first two approaches have been studied extensively by a number of scientists. These approaches share much in common. Both focus on skills development and emphasize the importance of role play and extensive practice in the skill development process. Both stress the importance of social influences, especially of peer pressures, in the initiation of tobacco smoking and other drug abuse behaviors, and they utilize similar techniques to help youth recognize and deal with these influences.

The two approaches differ in the extent to which they focus directly on substance use. The social inoculation approach, as described by Flay, focuses narrowly on substance use and the skills needed to resist social influences to use. The social skills approach, as described by Botvin and Wills, is based on the notion that other factors in addition to social influences explain drug use. Thus, this approach focuses on a variety of social skills in addition to social resistance skills. Many of these skills do not directly relate to substance use. By helping youth deal with other developmental challenges and, thus, feel more confident and comfortable with themselves, it is assumed that substance use can be prevented.

The third approach reviewed in this monograph, the cognitive-developmental approach described by Glynn, Leventhal, and Hirschman,

has not yet received the same degree of scrutiny as the other two. It is included in this volume because it provides a contrasting approach. It pays relatively little attention to the social influences which motivate the initiation of tobacco smoking. Instead it focuses on perceptions of use, experiences with initial use, and how cognitive processes regarding the smoking experience can be altered to discourage use.

This chapter will focus largely on the social inoculation and social skills research, which are similar not only in program focus but also in research methods employed and issues confronting the research. The purpose of this concluding chapter is to identify priorities for future research based on the state-of-the-art reviews in the preceding chapters.

As noted above, the social inoculation and social skills approaches to smoking prevention have been studied by a number of investigators. While early research in this area, begun in the mid 1970s, had a number of serious methodological flaws, the research has since become increasingly sophisticated. Data from a number of studies suggest that these approaches can prevent or delay tobacco use for as much as 3 years post intervention.

However, even the best of studies has limitations which confound interpretation of the results. Additional research is needed to address limitations in previous studies and to further develop and refine these prevention approaches. Research is also needed to confirm preliminary evidence of short-term effectiveness in preventing alcohol and drug use in addition to tobacco use.

The efforts of the social scientists reviewed in the preceding chapters suggest that the use of tobacco can be reduced among those adolescents participating in the preventive interventions. Moreover, the evidence suggests that similar interventions may be effective in reducing or delaying the onset of other drug abuse behaviors.

A number of challenges to the research appear to be shared among these projects. The remainder of this chapter will summarize these challenges and provide suggestions for future research directions. First, three general considerations concerning the magnitude, duration, and generalizability of program effects will be presented.

GENERAL CONSIDERATIONS

The first consideration deals with the magnitude of the intervention effect. As noted in the chapters by Botvin and Wills, and Flay, a number of authors have reported a reduction in adolescent smoking initiation of approximately 50 percent. Often the actual rates of initiation are not reported, leaving the interpretation of results unclear. Smoking has a low frequency onset in junior high school. Therefore, a reduction of 50 percent may reflect a program effect on a small proportion of the students exposed to the program. The immediate effect of the program on these students may ultimately be

less important than the potential effect of the program on the remainder. Thus, the interpretation of initial findings must be approached cautiously with many populations of young adolescents. It will often be necessary to follow subjects for an extended period of time before enough subjects are at risk to permit the assessment of the intervention's practical significance.

The second consideration addresses the question of how long is it reasonable to expect the program impact to be sustained. Unfortunately, the effects of the interventions appear to dissipate over relatively short periods of time. This has led investigators to add booster sessions in subsequent years to sustain behavioral change into the high school years. Such efforts, of course, require the expenditure of funds and resources, raising cost-benefit questions. Future studies should validate the need for, cost effectiveness of, and appropriate timing of booster sessions.

The final consideration deals with the potential for dissemination and the possibility of deterioration with general application. To date, junior high schools with well motivated, predominately white middle class students have been the site for the psychosocial intervention research. Moreover, these projects have usually been implemented by highly motivated scientists with the assistance of Federal funds not available to schools in general. Future research must go beyond the laboratory environment and address questions related to acceptance and utility of the preventive interventions by traditional educators in typical school settings.

SPECIFIC CONSIDERATIONS

The following concepts were culled from the preceding chapters and from the discussion during the RAUS review meeting. They represent not only a summary critique, but also a series of guidelines for future research. This section has been broken down into three categories: (1) major threats to the validity of existing research; (2) other issues that compromise the validity, interpretability, or generalizability of existing research findings; and (3) additional areas of research that warrant attention.

There appear to be three major threats to the validity of research on the psychosocially based interventions:

- o Attrition of subjects has been identified as a problem in a few studies, whereas many studies have not addressed this question at all. Attrition can present problems in terms of both the external and internal validity of a study. Research evidence suggests that subjects who are lost to the study represent a disproportionately large number of users of tobacco and other substances. Thus, high attrition rates may limit the generalizability of study findings to individuals who are at relatively low risk of use. If there is differential attrition among conditions, an apparent treatment effect may actually be due to the loss of high-risk subjects from the treatment group(s).

Existing data from completed studies should be examined to assess possible effects of attrition, looking both at rates of attrition and characteristics of subjects who have dropped out of each condition. Future studies and follow-ups on existing studies should carefully attend to subject tracking to minimize attrition. Without special efforts, attrition is likely to be an increasingly serious problem in longer term followups and in research with low-income populations.

- o As Flay has pointed out, previous research has not controlled for possible Hawthorne effects. While some studies have included comparison treatment conditions, these conditions have not been comparable to the experimental condition in terms of intensity or instructional methods. Research is needed which compares the effectiveness of psychosocial approaches with programs that are as similar as possible to the experimental treatment without including the program elements presumed to be responsible for change.
- o Especially of concern is the possibility that the special attention provided in the program would lead to an under-reporting of substance use rather than actual reduced levels of use, a concern regarding the validity of self-report data on substance use. Most of the social inoculation and social skills studies have included biochemical validation or a bogus pipeline procedure to increase honesty in self-report. However, the objective biochemical evidence has not been reported in many studies. Indeed the lack of sensitivity of commonly utilized testing procedures to detect experimental smoking makes validation of such smoking unlikely. Small-scale studies of biochemical validation procedures utilizing saliva cotinine, expired carbon monoxide, or combinations of measures should be conducted to assess further the validity of self-report data.

Additional methodological and analytical issues follow:

- o Most studies have utilized schools as the unit of assignment to treatment conditions. Until recently the number of schools assigned has been so small that random assignment does not assure comparability. Even in the larger studies the number of schools in any condition is often relatively small. Both initial differences among treatment conditions and differences that may emerge during the intervention or followup periods have received relatively little attention in most prior studies. Careful attention to and, where possible, control for differences need to be incorporated in all future studies.
- o Although schools have been used as the unit of assignment in most studies, individual subjects or classrooms have

been used as the unit of analysis. As Cook has pointed out, the unit of analysis should be the same as the unit of assignment for major analyses of program effects, although subanalyses of differential effects may require using subjects as the unit of analysis. Where numbers of units permit, schools should be used as the unit of analysis for major analyses. Where numbers do not permit this, the research must be considered quasi-experimental, and appropriate precautions must be taken to assess possible threats to the validity of the results.

- o Existing studies have paid relatively little attention to differential program effectiveness for various sub-categories of students. More needs to be known about the characteristics of students who respond favorably versus those who fail to respond to these programs.
- o Some studies have presented results only for subjects who have never smoked at pretest, whereas other studies have reported on nonsmokers and experimenters, and yet others have reported on all subjects. To understand the impact of an intervention program, it is essential that results be reported for all categories of pretest smoking. Comparison across programs would be facilitated by the use of similar categories across studies.
- o There is also considerable variability in the way in which the magnitude of program effects is reported. As noted above, a number of investigators report a percentage reduction in smoking initiation, e.g., the rate of initiation in the experimental group was 50 percent less than in the control group. Often not reported are the actual rates of initiation. Other investigators have reported differences in average cigarette consumption, e.g., a mean consumption rate of 2 cigarettes per month in the experimental group versus 4 cigarettes per month in the control group. Such reporting leaves unspecified the nature of the program impact. Are there fewer smokers or smokers using less? Is the program having a small impact on many subjects or is it having a substantial impact on a few? Greater clarity is needed in reporting the magnitude of program effect. Consistency across studies would again facilitate program comparisons.
- o With long-term followups the possibility of reactivity due to repeated measurement exists. Consideration should be given where feasible to controlling for repeated measurement by using subsamples on various measurement occasions.

Additional areas of research that have received insufficient attention thus far warrant further attention:

- o To date most of the research has been conducted with primarily white middle-class students. The extent to which these approaches may generalize to low-income and ethnic minority populations is unknown. Some research to address this question is currently in progress, but more is needed.
- o A number of the preceding authors have emphasized the need for increased attention to mediating variables in order to assess the processes by which program effects are achieved. For example, if it is hypothesized that smoking can be prevented by teaching refusal skills, then the extent to which such skills are learned and utilized needs to be measured, and change in refusal skills must then be related to change in smoking behavior. Focus on mediating variables is necessary to determine if the program is working as it is presumed to work and to differentiate effective from ineffective program components. The technology to measure mediating variables needs further refinement with attention to avoiding monomethod bias in measurement procedures used. While the measurement of mediating variables should be a part of any large-scale field trial, small-scale studies of program components can help to refine intervention approaches economically.
- o In addition to the dissemination issues identified above, research also needs to address such questions as the acceptability of instructional methods to teachers, packaging of programs to facilitate adoption, and teacher training modules.
- o A few issues that have received attention in previous studies require further exploration. One concerns the critical age(s) for intervention and the appropriate content and approach for different grade levels. A second issue concerns peer leaders. There is some support for the use of peer leaders, but the evidence is not conclusive. Research is needed to clarify the value of peer leaders, characteristics of effective peer leaders, and roles appropriate to peer leaders versus teachers.
- o The interrelationship between intervention programs and the environments in which they occur has received relatively little attention. The environmental factors that contribute to the success or failure of an intervention should be consistently explored. Similarly, the effect of the intervention on the environment, especially on the social contingencies that support substance use or abstinence, should be considered. Measurement technology to accomplish this needs to be refined.
- o Emphasis on long-term followup in the social inoculation/skills research is laudable. Followups as long as 3 years post intervention have been conducted. However, since many programs target sixth and seventh graders, and since many

youth initiate smoking and other substance use during high school, students should be followed through the end of their high school careers. The higher levels of smoking by the senior year of high school will also permit a more effective use of biochemical validation.

- o Research has been conducted primarily with schools as the unit of assignment on the basis of two assumptions. One assumption is that a key element of the intervention is its influence on the school environment. The second assumption is that interaction between experimental and control subjects will contaminate program effects. Neither assumption has been tested. Given the implications of the unit of assignment on the cost and rigor of the research, these assumptions should be verified.
- o The social skills approach contains the essential elements of the social inoculation approach plus additional program elements. Class time required for this approach is considerably longer than for social inoculation. Research should compare these approaches to assess whether the additional program elements result in incremental effectiveness and whether there are differences in the target groups with whom the approaches are effective.

SUMMARY

For most youth, substance use appears to be the result, in large part, of social influences. Thus, teaching youth to resist these influences appears to be a reasonable approach to the prevention of use. However, it is not realistic to assume that all youth use for the same reason or respond to the same prevention approach. Moreover, identification of factors that act to promote or deter the transition from experimental or occasional use to abuse has not been addressed by these studies. While the social inoculation and social skills approaches appear to hold considerable promise for many youth, the research results indicate that a sizeable number of youth initiate and escalate use in spite of these programs. It appears that those youth who are most at risk for compulsive drug use are those who use drugs for reasons other than social influences (Robins and Przybeck 1985).

Thus there is a need not to focus on any single prevention approach, but to explore multiple strategies. Identifying effective prevention approaches also requires the ability to target programs--to identify which types of individuals are effectively reached with a specific approach. Alternative programs, such as the cognitive-developmental approach described by Glynn, Leventhal, and Hirschman, warrant additional attention.

A host of areas needing further research have been identified above. While the RAUS review participants did not establish priorities among areas, some attempt to do so is necessary since funds available for research are limited. Most important is the

need for research on the long-term efficacy of social inoculation and social skills programs in the prevention of alcohol and other drug abuse, research that would extend through high school. Another important priority is the assessment of program effects with low-income and ethnic minority populations. Also of importance is research designed to control for possible Hawthorne effects. Of somewhat less importance is research to compare social inoculation and social skills programs or research to identify the effective components of interventions.

A number of points raised by the authors serve as guidelines for all future research. Important among these are: (1) attention to attrition; (2) differences among schools; (3) differential effects for various groups; and (4) the ways in which data are analyzed and reported.

As has been noted in the preceding chapters, even the best of studies have limitations that confound interpretation of the results. It is important to recognize, however, that if the short-term results reported by these groups of investigators are of sufficient magnitude, and if they are replicable and sustainable, it could justify dissemination of preventive intervention programs to the educational community. With the relative success of the current research efforts, the development of more sensitive measurement techniques and noninvasive verification measures, and the ability to identify and strengthen intervention components, it is likely that future efforts will result in a substantial reduction in the incidence and prevalence of drug abuse among children and adolescents.

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